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IMMEDIATE MEMORY AND SIMULTANEOUS STIMULI

BY

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When two sets of digits are presented simultaneously one to one ear and one to the other, the subject may be asked to write them down from immediate memory beginning with those on an ear chosen by the experimenter. He can do this, but any mistakes are usually amongst those on the ear prescribed for later response. If the subject does not know the order of recall until after stimulation, he either shows low efficiency with the same serial order effect, or else normal efficiency with an altered serial order effect.

When the order of recall is known in advance, efficiency is also reduced by altering the time of presentation of the last items to be recalled; and also by inserting irrelevant items. All these facts are consistent with a particular theory of immediate memory.

I

INTRODUCTION

If a memory span experiment is performed in which three digits are delivered to the sensory channel and three more simultaneously to another channel, then all six may be repeated from immediate memory; but normally the three from one sense will appear in response before the three on the other. This has been shown for several combinations of sensory channel (Broadbent, 1956) and appears to be a general mode of behaviour.

Behaviour of this type implies, firstly, some mechanism within the subject which can only pass information successively. We may call this the (p) mechanism. There must also be, however, another system, which we call the (s) mechanism, and which can contain simultaneously the information from two channels. This is merely a logical consequence of the observed behaviour, as is the fact that the (s) mechanism must lie on the sense-organ side of the (p) mechanism. The two systems may be physically related despite their logical distinctness; the letters (p) and (s) are derived from "perceptual" and "storage" respectively, but these latter words may have unwarranted implications to some people and the letters are therefore preferable.

It may fairly be presumed that the (p) system is the same as that operative in ordinary memory-span experiments, in which the information both enters and leaves the nervous system successively. The existence of the (s) system, however, raises a point of some interest. Is this system operative in ordinary immediate memory, or is it merely an emergency device introduced for the special case in which excess information arrives while the (p) system is already fully occupied with another channel? Are we in fact dealing with a new variety of short-term memory or rather with a new technique for dissecting the same processes involved in ordinary memory span?

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The two experiments in this paper show that the latter alternative is plausible, although not completely certain. The first experiment attempts to detect the role of the (p) system by giving different instructions about the order of recall; the second experiment to show the effect of time delays and irrelvant interpolated stimuli upon information in the (s) system.

II

Experiment I

Varying Recall Order

It appears to be the case that the first material to pass through the (p) system is better reproduced than the later material. This statement is justifiable, firstly, by the long-established fact that greater difficulty is met in the second half of a list presented by conventional means for immediate reproduction (Robinson and Brown, 1926).

Secondly, Kay and Poulton (1951) have shown the same serial order effect even when the subject does not know in advance what the order of recall is to be. But this depends on the information being recalled in the same order as it was presented, that is, on its entering and leaving the (p) system in the same order: if the order is reversed the two halves of the list become equal in difficulty. Brown (1954) has shown that the first recalled items reduce the efficiency of later recalled items, time of presentation being equal, and it seems therefore that reversal of order between stimulus and response cancels out the two effects.

Thirdly, this greater efficiency of the first items through the (p) system applies to simultaneous presentation as well as to conventional successive presentation. In the whole series of experiments on successive response to simultaneous stimuli, the writer has found that when the subject is allowed to choose his own order of recall, the first sensory channel to be dealt with shows fewer errors than the second. Thus for example in the High and Low Frequencies Experiment of Broadbent (1956), nine subjects showed this type of performance and none gave an opposite effect, so that $p < 0.01$.

Fourthly, the same serial order effect appears with simultaneous stimulation even when the subject is told in advance which sense he is to handle first. Brown (1954) has shown this for visual material, and the results to be given below show it for auditory material.

Now, our problem is to decide whether the (s) system, necessary for the simultaneous stimulation case, is operating even in successive stimulation cases. If it is, and the (p) system is merely a later stage of the normal progress of information through the man, it should be possible to prescribe a particular order of recall at any time before recall: even after stimulation. The (p) system could then take in the information in the required order, and response would show normal efficiency and a normal serial order effect.

On the other hand, if the (p) system provides the normal route between input and output, and the (s) system is merely an extra store for excess information, then one or the other channel would deliver its information to the (p) system immediately on stimulation. An instruction, to recall in a particular order, arriving after stimulation, could not therefore affect the order of input to the (p) system, but only the order of output. The difference between the two orders would give, by analogy with the results of Brown or of Kay and Poulton, an absence of serial order effect.

The preceding argument may be put in more everyday terms to make easier reading, though with the usual unwarrantable over-tones. We can listen to only one

voice at once, and the first words we hear are the best recalled. Can we sit passively while simultaneous messages arrive, and then listen to them in an order prescribed by the experimenter? Or must we listen to one of them as soon as they arrive? In the latter case we could not choose to listen first to the prescribed message, and will recall it no better than the other.

(a) Method.

Only two experimental conditions were employed, which may be termed *known* (k) and *ambiguous* (a). In the (k) condition, lists of six digits were presented, three to one ear and three different ones to the other ear. A two-channel Ferrograph tape recorder was used to do this, and the situation was identical to the binaural condition described by Broadbent (1956). The subject was, however, instructed that he was always to write down all digits from one ear before any from the other; and that the ear which was to be written first on each occasion would be chosen by the experimenter. Two pieces of paper, bearing the words "Right" and "Left" respectively, were laid in front of the subject: and the experimenter pointed to one of them before each binaural presentation. The subject then received the list, and wrote down first the three digits on the side indicated and then the three on the other side. In the (a) condition the situation was exactly the same except that the experimenter did not indicate the ear to be written down first, until after the digits had been presented.

The (k) and (a) conditions have been deliberately named after two closely similar conditions used by Brown (1954) with visual material: but two differences should perhaps be noted. One of these is that in Brown's (a) condition there was a possibility that some of the material might not need to be recalled at all, which is not the case in the present (a) condition. The other is that Brown required his subjects to react to each stimulus as it was received, so that they must in some sense have perceived even material which they could not recall; this also is not the case in the present work.

Thirty-two naval rating subjects were used, divided into four equal groups. Each of them received two days of testing: on each day five lists given under the simple auditory condition preceded and followed ten lists under the condition to be tested, to act as a buffer.

Group I received the (k) condition on both days.

Group II received the (a) condition on both days.

Group III received the (k) condition on the first day and the (a) condition on the second day.

Group IV received the (a) condition on the first day and the (k) condition on the second day.

(b) Results.

The effect of the (k) and (a) conditions is different on the first day of the experiment from that on the second day: it is for this reason that a four-group experimental design was required. Figures 1 and 2 show the results: Figure 1 gives the percentage of lists correctly reproduced, while Figure 2 is concerned with the relative frequency of errors on the digits written down last as opposed to those written first. A numerical index of the balance between the ear indicated by the experimenter's instructions and the other ear, was obtained by taking the simple difference between the number of correct responses on the two ears. This quantity is that plotted in Figure 2, the positive sign indicating that more correct responses were made on the side written down first.

If we consider only the first day of the experiment, it is clear that the two conditions
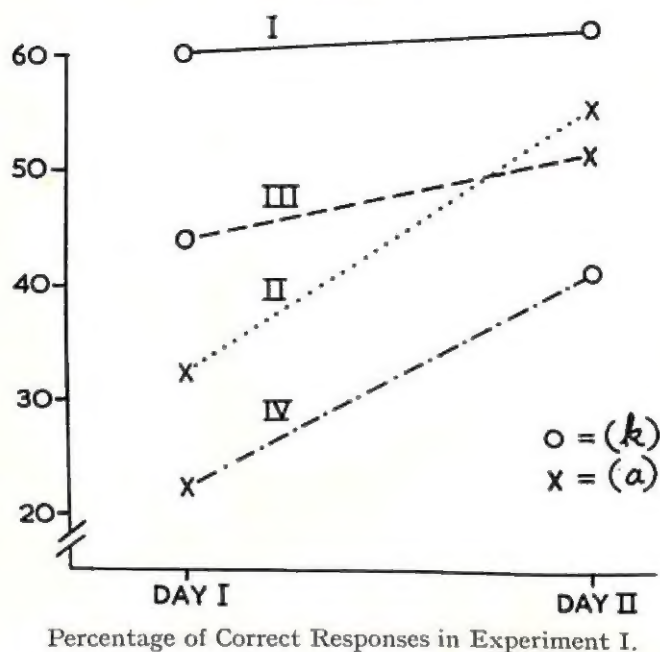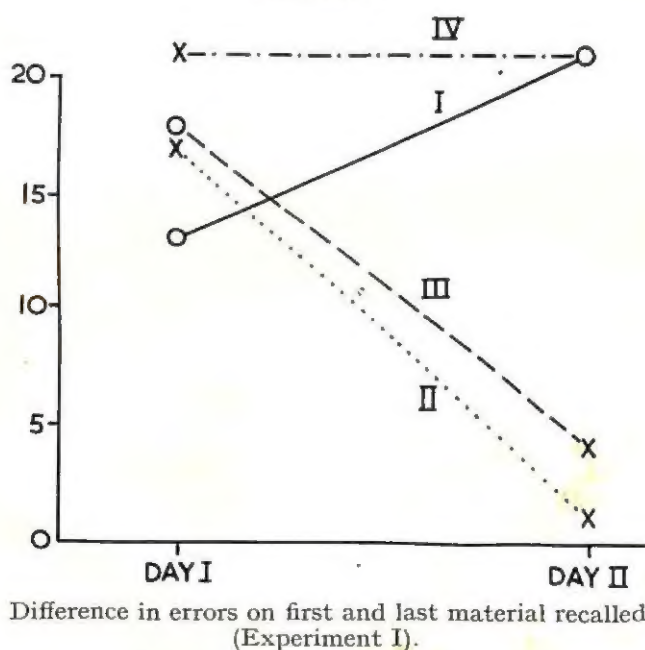
FIGURE 1

Percentage of Correct Responses in Experiment I.

FIGURE 2

Difference in errors on first and last material recalled
(Experiment I).

do not differ appreciably from one another in relative performance on the two ears:
using the ranking technique of Whitfield (1947) on the quantity drawn in Figure 2,
the critical ratio <1. (It should be noted here that a proportion score instead of
a simple one gives similar results—the absence of any apparent difference between
conditions is not due to the different level of general efficiency.) Under both condi-
tions there is in fact a marked tendency to record the ear indicated by the experimenter

more accurately than the other ear. In each condition only one subject showed the opposite effect, so that the significance is easily better than the 0·01 level.

On the second day, however, a very different state of affairs appears. The two conditions differ markedly in the balance of the two ears (critical ratio = 2·52 so that p <0·02). While the (k) condition still shows a preference for the ear indicated by the experimenter (no subjects giving an opposite effect, so that binomial theorem again gives p <0·01), the (a) condition no longer does so significantly. Even using a t test, as more sensitive than those applied in the other cases, $t < 1$.

So we must conclude that our original analysis posed a false alternative; incoming information may pass straight into the (p) system, excess being held in the (s) system, but it is also possible for all the incoming information to enter the (s) system and pass to the (p) mechanism only at a later time. The latter is the strategy adopted early in practice, and the former later in practice.

Why should the listener come to adopt the more active strategy of passing one set of digits straight into the (p) system? Because, as Figure 1 shows, the total number of successful recalls is greater with that strategy. On the first day the (a) condition gives lower efficiency than the (k). The difference between Groups I and III on the one hand, and Groups II and IV on the other is significant at the 0·01 level, critical ratio = 2·60. On the second day the two conditions are almost equal in general efficiency (critical ratio <1). It may be noted also that there is no difference on that day between subjects who received the (k) condition on Day 1 and those who received the (a) condition: for the difference between Groups I and III on the one hand and Groups II and IV on the other, critical ratio <1. It is also clear that performance under the (k) condition is much the same on either day: it is the (a) condition which changes markedly.

This difference means that the strategy of holding all the information in the (s) system for a time can hardly be that normally used in immediate memory. Although it is possible, it is less efficient than the alternative course of starting to deal with information successively even during its arrival. In ordinary immediate memory information probably enters the (p) system at once without passing first through the (s) system.

(c) *Discussion.*

This result, which seems to reveal the (s) system as a peculiar function found only in simultaneous stimulation, immediately raises a fresh problem. If it is more efficient to avoid the use of the (s) system, a memory span obtained using two simultaneous channels should be less than a span obtained using orthodox successive presentation. But it was shown by Broadbent (1956) that this is not so when the eye and ear are the two sensory channels,

An obvious way of reconciling this conflict is to suggest that the (s) system does play a role in ordinary immediate memory, though not that of holding the first items of information prior to their entering the (p) system.

Suppose, for example, that all information not actually in transit through the (p) system was stored in the (s) system, and that the latter could hold any particular item only for a limited time. In ordinary immediate memory each digit would first enter the (p) system, pass through into the (s) and stay there while all the other digits were dealt with, then pass through the (p) system again and so on. The length of the memory span would be set by the number of digits which could pass the (p) system before the first one vanished from the (s) system. This span would be the same whether simultaneous or successive stimulation was used, but recall would be impaired

if the (p) system did not begin to accept items until some time after the first stimuli had arrived and entered the (s) system.

This statement is derived from a view suggested by Brown (personal communication) as a result of his own experiments. He puts forward the possibility that, in immediate memory, each item of the material is alternately rehearsed and stored as a memory trace. The trace fades with time, but is restored by rehearsal, and the length of the span is thus set by the time taken to rehearse all the material before the first trace has declined before threshold. This view may be translated into our language of (s) and (p) systems in the manner of the previous paragraph, and it certainly harmonises well with the results of the present experiments. The chief addition necessary to Brown's view is the postulate that the trace or (s) system to which the information is passed after the (p) system is the same as the (s) system in which information is stored before the (p) system when stimuli arrive simultaneously. It is hard to think of a psychological experiment which would prove this, and it may require physiological techniques.

It should be noted that in those of Brown's experiments which have been published (Brown, 1954) subjects were required to respond during presentation, as noted above. This means that his results do not require an (s) system before the (p) system, which last would presumably operate during presentation. It may be because of this difference in technique that he did not report the type of performance found here for (a) conditions with low levels of practice: that is, greater efficiency on the material picked out by the experimenter for prior recall. An alternative reason for this discrepancy is that his subjects were practised for a day before results were taken. With this exception all his results under conditions similar to those of the present experiment are broadly similar.

In summary, then, this experiment shows that it is usual to deal with stimuli successively while they are actually arriving. Although it is possible to take a more passive attitude and wait till after the stimuli have all arrived before beginning to deal with them successively, such an attitude is inefficient. This produces a discrepancy in that simultaneous delivery of stimuli is not less efficient than successive delivery, and the discrepancy may be reconciled by supposing that the type of immediate memory process found in this experiment is one in which information is rapidly lost as time goes by: and that this process is also involved in ordinary memory span situations. We are thus left with an acute interest in the effects of the time for which information stays in the (s) system.

III

EXPERIMENT II

(DELAY BEFORE RESPONSE TO THE SECOND CHANNEL)

The results of the last experiment give some interest to the effects of lapse of time between stimulus and response. In conventional immediate memory experiments, a delay of a few seconds between presentation and recall makes little difference to efficiency. This is a matter of common observation, as for example in looking up and then dialling a telephone number; and Brown (1954) mentions an unpublished formal experiment on the subject. But in such cases rehearsal is, of course, likely to occur; in the terms introduced above, ordinary immediate memory involves the (p) mechanism, while it is the (s) mechanism which may show effects of lapse of time. Consequently an experiment was designed using simultaneous stimulation.

(a) *Method.*

Binaural presentation was used throughout, and under all conditions the subjects had to write down firstly six digits presented to the right ear and secondly two digits presented to the left ear. The different conditions varied in the time at which the two digits were presented to the left ear, and in the number of other irrelvant digits also presented to the left ear. Table I gives the various possibilities in convenient form.

In the A condition the two left ear digits arrived only towards the end of the right-ear series, while in the B condition the left-ear digits were in the middle and in the C condition at the beginning of the right-ear series. Thus a progressively longer length of time was introduced between presentation and recall of the left-ear digits. These three conditions were presented both at a ½-second and at a 1-second speed of speaking, in order to distinguish lapse of time as such from amount of intervening activity of the subject. It was hoped that, in view of the results of Broadbent (1956), even a 1-second rate would not allow a shift of attention to the left ear until all the right-ear digits had arrived: as will be seen, this hope was not altogether fulfilled.

In the D, E and F conditions the left-ear digits were always in the earliest position, and irrelevant digits, not to be recalled, were presented on that ear after them. In the D condition, there were no such irrelevant digits, in the E condition two, and in the F condition four. A ½-second speech rate was used.

TABLE I

CONDITIONS OF EXPERIMENT II

	A	B	C
Right Ear ..	736454	736454	736454
Left Ear ..	12	12	12
	D	E	F
Right Ear ..	736454	736454	736454
Left Ear ..	12	1295	129587
	G	H	I
Right Ear ..	736454	736454	736454
Left Ear ..	958712	951287	129587
Response for all conditions 736454		12	

In the G, H and I conditions, there were always six digits on the left ear just as on the right: the two to be recalled were the last two in the G condition, the middle two in the H condition and the first two in the I condition. Again a ½-sec. speech rate was used.

It will be noted that the C and D conditions on the one hand and the F and I conditions on the other are objectively identical: but they are listed separately to make the desired comparisons more clear. They were also presented separately to the subjects in order to make the instructions simpler.

One group of twelve naval rating subjects received ten presentations under each of the A, B and C conditions; first at one speech rate and then at the other. Half the subjects started with each rate and the order of conditions for each rate was rotated on the Latin Square principle.

A separate group of twelve naval rating subjects received the other six conditions,

half the subjects starting with the D, E and F conditions and half with the remainder. The order of conditions within each set of three was rotated on the Latin Square principle.

(b) *Results.*

Table II gives the percentage of correct responses under each condition. On the whole these support the theory that the (s) system becomes very inefficient if expected to hold information for longer times. That is, a delay between presenting a stimulus on a second sensory channel and allowing response to it produces a marked effect, in contrast to the effect of delay on ordinary immediate memory.

TABLE II

PER CENT. COMPLETELY CORRECT RECALLS
IN EXPERIMENT II

A	B	C
	Fast Speech Rate	
44	30	28
	Slow Speech Rate	
43	30	43
D	E	F
37	5	3
G	H	I
19	1	3

At the fast speech rate, it is clear that the A condition (least delay on second channel) is more efficient than the B and C conditions; which are about equal. The differences A-B and A-C give $t = 2.29$ and 2.91 respectively, 0.05 level $= 2.20$. Thus it appears that increasing the length of time for which material is in the (s) mechanism reduces the accuracy of recall; but the attempt made to separate time as such from amount of intervening activity by varying the speech rate meets some difficulty.

In the slower speech rate conditions the time between stimulus and response was increased without any increase in the amount of intervening activity. It was hoped that if time as such was the more important variable performance would be worse; while if intervening activity was the major factor, performance would be the same. In fact conditions A and B give performance closely similar at both rates, but condition C (longest delay) shows an unexpected high level of efficiency at the slow rate, higher than the B condition. It is therefore difficult to draw any conclusion about the importance of time as such as opposed to interpolated activity.

As such an unexpected result sheds some doubt on our analysis, some explanation should perhaps be offered. It seems conceivable that the slow presentation rate allows the listener to take the information into the (p) system in an order contrary to the instructions, as was shown for slower rates by Broadbent (1956). Although that paper showed that a number of items could not be handled alternately on two channels at a 1-second presentation rate, a single shift might be possible at that speed. In the C condition this would mean that the listener could listen to the left ear before the right and then invert the order again in recall. This suggestion may be supported

by considering once again the effect of order of presentation on recall. In orthodox memory span experiments, the last item presented is recalled better than the one before it (Robinson and Brown, 1926). So also in the $\frac{1}{2}$-second rate conditions the last of the two left-ear digits is better recalled than the one before it ($t = 6\cdot11$, $0\cdot001$ level $= 4\cdot44$). But in the 1-second rate presentations this is not so ($t < 0\cdot1$). This certainly shows some difference in method of performance at the two speeds, and agrees well with the suggestion that the last items recalled are not, at the slower speed, the last items perceived.

As some change in method of performance is thus taking place at the slow speed, and since that condition undoubtedly slackens the control of the experiment, we need not reject our analysis because of the unexpected result. Although no conclusion about the relative effects of time and of interpolated activity is possible, the distinction is perhaps an artificial one in any case. At least it seems clear that one or the other factor reduces the efficiency of recall of material which is held in "preperceptual storage" for a few seconds. This is consistent with the views advanced about the last experiment.

There is no doubt, however, about the effect of interpolated material on the left ear itself, as opposed to the right ear. Conditions F, H and I are virtually impossible, the number of correct reproductions being negligible. Condition E (only two irrelevant digits) is significantly better than zero at the $0\cdot05$ level ($t = 2\cdot58$), but it is far worse than the D condition which has no interpolated material ($t = 3\cdot80$, $0\cdot01$ level $= 3\cdot11$). The presentation of irrelevant material on the same sensory channel as the material in the (s) mechanism definitely lowers efficiency. It does not seem that the irrelevant material must arrive after the presentation of the relevant stimuli for this effect to appear. Condition G gives a reasonably high score, but it is due to only four subjects, so that the significance of the difference from zero is in doubt ($t = 2\cdot14$, $0\cdot05$ level again $= 2\cdot20$). Furthermore condition G is significantly worse than condition A using the method of Whitfield (1947). Critical Ratio $= 2\cdot39$.

A final point should be made about the effect of irrelevant material. It was held by Broadbent (1954, 1956) that the greater difficulty of binaural presentation, as opposed to conventional memory span conditions, was due to a confusion which is "perceptual" and not related to the temporary storage of some information. It did not imply that some extra inefficient mechanism was being used by the listener to deal with simultaneous stimulation. This perceptual confusion is, of course, essential to the argument which was developed on the results of Experiment I: since that argument assumed that memory span was the same in bisensory and conventional cases, as was found in the Eye and Ear Experiment of Broadbent (1956). The confusion arising with binaural presentation appears directly in the present experiment. The A, B and C conditions (no irrelevant digits) gave almost equal performance on the right ear, and the total number of correct responses was decided by the efficiency of performance on the left ear. But as irrelevant material is introduced on the left ear, right-ear performance drops off. On that ear alone, eleven subjects did worse on condition F than condition D, the remaining subject showing no difference, so that $p < 0\cdot01$. This result contrasts strikingly with the high level of efficiency, which can be reached when listening to one ear and ignoring irrelevant material on the other ear entirely (Cherry, 1953). Apparently the fact that some of the left-ear items are relevant in this experiment makes additional irrelevant left-ear items distracting.

It should also be noted, since the right-ear performance is affected by irrelevant items, that the results stated above are not due simply to the effect of right-ear performance on overall efficiency. All results apply equally to the proportion of correct right-ear responses which also produce correct left-ear responses.

IV

GENERAL DISCUSSION

So far as the nature of immediate memory is concerned, results from the present type of experiment cannot give a completely unequivocal answer; but they seem to support a view of immediate memory in which a series of items pass successively through a (p) mechanism into an (s) one, and back again: the probability of recovering an item from the (s) mechanism falling off with time. To summarise the argument, delayed operation of the (p) mechanism in the Varying Recall Order Experiment is associated with low efficiency; one possible explanation of this is that the (s) mechanism is responsible, and the Delay in Second Response Experiment does in fact show that a delay in recovering items from the (s) mechanism reduces efficiency. But if this is so, then the (s) mechanism is operating in ordinary immediate memory since bisensory and conventional memory spans are equal. The main weaknesses in this argument are that some alternative explanation of the results of the Varying Recall Order Experiment may be possible: and, less probably, that immediate memory might not involve the (s) mechanism, but might involve some other factor producing an inefficiency exactly equal. However, the conclusion is consistent with that drawn by Brown from his rather different experiments (personal communication). Furthermore, there are other immediate memory experiments which point In the same direction. These are those based on information theory calculations (Aborn and Rubinstein, 1952; Pollack, 1953). The length of a list of items may be varied independently from the amount of information contained in the list as a whole. When this was done by Aborn and Rubinstein, they found that the amount of information in the maximum memory span remained constant for short lengths; but at the longest lengths the number of items recalled was the same, while the amount of information declined. Pollack found that the percentage of information lost between presentation and recall varied most directly with the number of items rather than the total information or the information per item. Miller (1956) places even more emphasis on the number of items, arguing that a memory span is about seven items regardless of the information per item. This importance of the length of list is quite consistent with the view advanced by Brown. The information content of the items might, however, also be expected to have some effect at extreme values since the reason for the successive passage of material through the (p) mechanism is presumably that the latter has a limited capacity in the information theory sense. In that case signals conveying very little information might give rise to simultaneous responses when presented at the same time: but for complex signals simultaneous stimuli can only give successive responses.

Finally, we may return to the point raised by Broadbent (1956): is attention unitary or divisible? As was said in that paper, the antithesis is a false one. Some processes can be simultaneous, others must be successive. The results of the present paper show, however, how certain experimental variables in Mowbray's experiments helped him to achieve the valuable result of isolating the (p) system, the process which can only deal with information successively. In one experiment (Mowbray, 1953) he employed meaningful passages of prose of some length as his simultaneous stimuli: which would, in our terms, occupy the (p) system with one sensory channel so long that the material reaching the (s) system would be beyond reach. In another experiment (Mowbray, 1954) two short phrases were presented simultaneously, but were embedded in large quantities of irrelevant material. As was shown in Experiment II of this paper, this would make recovery of the information on one channel very difficult by the time the (p) system had finished dealing with the other channel.

Mowbray was thus able to set limits to the assumption that separate chains of events linking stimulus to response can co-exist. Yet it can easily be seen that the existence of the (s) mechanism makes it possible for two everyday tasks to be carried on during the same general period of time, as long as the average rate of flow of information is not too high: and so to give rise to scepticism such as that of Hebb (1949) about a concept of indivisible attention.

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A STOCHASTIC DESCRIPTION OF THE LEARNING BEHAVIOUR OF AN INDIVIDUAL SUBJECT

BY

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A stochastic process for the description of learning behaviour in a two-choice situation, a special case of a family of processes given in an earlier paper, is introduced. It can be used to describe individual learning data, including both the choice sequence and the latency of the choices within the description. This has the advantage that parameter estimates may be based separately on the latencies and on the choice sequence, and the agreement between these estimates can be taken as an indication of the validity of the stochastic description.

The process is fitted to fifty learning trials of an albino rat. Only two parameters are employed in the description of the sequence of choices made by the subject, and a further one to include the response latencies. No generality is claimed for the success of the particular process employed in this one example, which is given only as an illustration of the possible value of the method.

INTRODUCTION

A description of the learning behaviour of an individual subject by means of a mathematical model appears to have been first attempted by Gulliksen (1934). His model was essentially a stochastic one, and was in principle capable of relating the probability of a subject making a correct response at any given trial to the past experimental history of the subject. Mathematically, however, the behaviour of the stochastic model was summarized by a learning-curve equation and the advantages inherent in a probabilistic approach were consequently lost. More recently, Bush and Mosteller (1955) have elaborated one particular kind of stochastic model for the description of learning. This approach has also been employed, within a particular theoretical context, by Estes (e.g., 1954). In application, these stochastic models have been fitted to group data. This procedure carries with it certain disadvantages, and does not fully exploit the possible scope of stochastic descriptions of behaviour.

The above points have been considered in more detail elsewhere (Audley & Jonckheere, 1956) and the possibility of a true stochastic description of individual data, and the problems involved in attempting this, were also discussed. In the same paper a family of stochastic processes for learning was introduced. This family includes among others the Gulliksen model cited above. In a further paper (Audley, 1957) a simple means was suggested by which the latencies of responses and the probability of their occurrence might be simultaneously described. The present paper provides an experimental illustration of the descriptive approach advocated in these papers and is concerned with the learning of a position habit in a simple two-choice situation by an albino rat.

THE STOCHASTIC PROCESS

The process employed in the present paper is a very simple case of the general process developed in the paper by Audley and Jonckheere. The probability of a

success on the nth trial, given k_n previous successes, which will be symbolized as $P(n/k_n)$, is given by the equation

$$P(n/k_n) = \frac{\rho + k_n\alpha}{1 + k_n\alpha} \qquad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \qquad (1)$$

where ρ is the initial probability of a correct response on the first trial and α is a parameter associated with the increase in probability of a correct response. It is therefore assumed that only reward has any influence on learning. This is also a special case of the model suggested by Gulliksen.

Equation (1), however, can be derived from a stochastic process which includes a description of response times. This process assumes that on any given trial the probability of a correct response occurring in a small time interval $(T, T + \Delta T)$ is $r\Delta T$, and the probability of an incorrect response is $w\Delta T$, where r and w are hypothetical mean rates of responding. It is further assumed that the hypothetical rate of responding associated with the correct response is a linear function of the number of previously rewarded correct responses. Hence, on the t_{th} trial

$$r_t = r_1 + k_t a \qquad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \qquad (2)$$

$$w_t = w_1 \qquad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \qquad (3)$$

where r_1 and w_1 are the initial rates of responding on the first trial, r_t and w_t are these rates on the t_{th} trial and a is a parameter associated with the change induced by reward. k_t is the number of rewarded correct responses previous to the t_{th} trial.

Bearing in mind that in most two-choice situations an experimental trial is terminated as soon as one of the responses occurs, it can be shown that the probability of a correct response terminating the t_{th} trial in the time interval $(T, T + \Delta T)$ is given by the equation

$$P_t(T) = e^{-(r_t + w_t)T}r_t\Delta T \qquad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \qquad (4)$$

Similarly, the probability of an incorrect response terminating the trial in this time interval is

$$Q_t(T) = e^{-(r_t + w_t)T}w_t\Delta T \qquad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \qquad (5)$$

Furthermore, the probability of a correct response on the t_{th} trial is

$$P(t/k_t) = \int_0^\infty e^{-(r_t + w_t)T}r_t dT = \frac{r_t}{r_t + w_t} \qquad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \qquad (6)$$

given k_t previous successes.

Using equations (2) and (3) we have

$$P(t/k_t) = \frac{r_1 + k_t a}{r_1 + k_t a + w_1} \qquad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \qquad (7)$$

If we then put $\rho' = \dfrac{r_1}{r_1 + w_1}$ and $\alpha' = \dfrac{a}{r_1 + w_1}$, we derive equation (1).

The theoretical distribution of the response latencies at each trial can also be derived. In particular the mean latency of responses on the t_{th} trial is given by the equation

$$M_t = \int_0^\infty e^{-(r_t + w_t)T}(r_t + w_t)TdT = \frac{1}{r_1 + w_1 + k_t a} \qquad \cdots \qquad \cdots \qquad (8)$$

There are, therefore, two parameters required to describe the sequence of choices made by the subject, namely ρ and α, and a further parameter, say w_1, to include the response latencies in the description. Given these three parameters the remainder are fixed.

The equivalence of equations (1) and (7) suggests that estimates of the parameters ρ and α can be carried out separately, using (a) the sequence of responses made by the subject and (b) the latencies of these responses. Good agreement between these estimates might be taken as a first indication of the adequacy of the stochastic description, although the assumption of an exponential distribution of response latencies contained in equations (4) and (5) needs further assessment.

ESTIMATION PROCEDURE

The choice sequence

The result of the t_{th} trial may be symbolized by a variable X_t $(t = 1, 2, \ldots, n)$. Thus $X_t = 1$ if a correct response is made on the t_{th} trial, $X_t = 0$ if an incorrect response occurs. Maximum likelihood estimates of the parameters ρ and α are given by the equations

$$\sum_{t=1}^{n} X_t(\rho + k_t\alpha)^{-1} = \frac{n-k}{1-\rho} \qquad \qquad \qquad \qquad \qquad (9)$$

and

$$\sum_{t=1}^{n} X_t k_t(\rho + k_t\alpha)^{-1} = \sum_{t=1}^{n} k_t(1 + k_t\alpha)^{-1} \qquad \qquad \qquad (10)$$

where k is the total number of successes in the n observed trials.
An iterative procedure is required for the solution of these equations, and details of this will not be given here.

The response latencies

If T_t is the response latency at the t_{th} trial, then maximum likelihood estimates of the parameters r_1, w_1 and a are given by the equations

$$\sum_{t=1}^{n} T_t = \sum_{t=1}^{n} X_t(r_1 + k_t a)^{-1} \qquad \qquad \qquad \qquad \qquad (11)$$

$$\sum_{t=1}^{n} T_t = \frac{n-k}{w_1} \qquad \qquad \qquad \qquad \qquad \qquad (12)$$

$$\sum_{t=1}^{n} k_t T_t = \sum_{t=1}^{n} X_t k_t(r_1 + k_t a)^{-1} \qquad \qquad \qquad \qquad (13)$$

An iterative solution of these equations is again required.
Combining equations (11) and (12) we have

$$\sum_{t=1}^{n} X_t(r_1 + k_t a)^{-1} = \frac{n-k}{w_1} \qquad \qquad \qquad \qquad \qquad (14)$$

Multiplying both sides of (14) by $(r_1 + w_1)$ and putting $\dfrac{r_1}{r_1 + w_1} = \rho'$ and $\dfrac{a}{r_1 + w_1}$ $= \alpha'$, we find

$$\sum_{t=1}^{n} X_t (\rho' + k_t \alpha')^{-1} = \frac{n-k}{1-\rho'} \quad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \quad (15)$$

This is equivalent to equation (9), which shows that there is the same relation between the estimates based on the choice sequence and those based on the latencies. However, the actual values of the parameter estimates based on the two kinds of data depend on further independent functions. In the case of the latency data, for example, this further relation is

$$r_1 = \frac{k - a \sum_{t=1}^{n} k_t T_t}{\sum_{t=1}^{n} T_t} \quad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \qquad \cdots \quad (16)$$

where k is the total number of successes in the entire n trials.

It should be borne in mind that the stochastic process developed here assumes that the response latencies are distributed exponentially at each trial (although other distributions could be assumed within the present method) and hence near zero response times are considered possible and indeed likely. In practice, however, it is obvious that there will be some sizeable minimum response time. It is therefore suggested that the process should be fitted not to the actual response times but to the difference between these and the minimum observed time. This procedure is followed in the experimental illustration given below.

Once the parameter values are determined, the theoretical mean latencies, M_t, are given by equation (8). The ratio of the observed response times to the theoretical mean times, i.e. $R_t = \frac{T_t}{M_t}$, should be distributed exponentially and e^{-R_t} should have a rectangular distribution in the region (0,1), which provides a basis for testing the adequacy of the theoretical description of the observed latencies. It is possible, of course, that there might be systematic variations from the theoretical description which are obscured by the above statistical treatment so it is further suggested that a plot of e^{-R_t} against t, the trial number, should be used to detect these.

EXPERIMENT

Apparatus

A schematic plan of the apparatus is given in Figure 1. A is the starting compartment. BB is a swing door, which when raised gives access either to a single central raised button in the floor, L_c, or to two buttons L_1 and L_2. A light pressure on these buttons by the animal subject causes the buttons to retract to floor level.

FIGURE 1
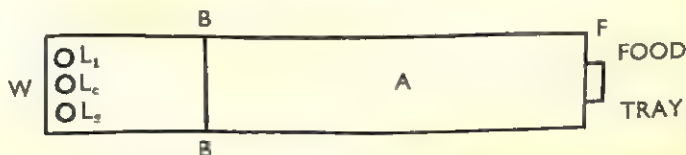
If the "correct" button is pressed, food pellets are delivered at F. If the "incorrect" button is pressed, a sound similar to the food delivery mechanism is produced. In

either case, the animal has been trained to return to A and nose the flap of the food tray, which causes the door BB to close behind the animal. The door reopens after a fixed period of time selected by the experimenter. As the door opens, a kymograph is set in motion, recording the button pressed and the time elapsing between the opening of the door and the pressing of this button. Observation windows are placed at W and in the roof of A. The apparatus is entirely automatic.

Subject

The subject was an albino rat approximately nine months old at the commencement of the experiment. This was the first animal run in the apparatus described above.

Procedure

Three weeks of daily handling and adjustment to a feeding cycle (two hours *ad libitum* feeding every twenty-four hours) preceded the experiment proper. Details of the remaining experimental procedure are given in the following diary.

Days 1 and 2. Animal placed in the apparatus, with food in the tray, for ten minutes.

Day 3. The food tray was initially empty and the food delivery mechanism was actuated every thirty seconds until twenty pellets had been taken.

Days 4–7 inclusive. Same procedure as day 3, but the number of pellet deliveries was 30, 40, 50, 50, respectively, with approximately thirty seconds between deliveries.

Day 8. The animal was placed in compartment A, door BB opened and the central button L_c raised, smeared with wet mash. This was repeated for three trials but for seventeen more trials the button was not baited. Thirty seconds elapsed between the nosing of the food tray and the re-opening of the door BB.

Days 9–11. Same procedure as Day 8, except that 30, 40, 50, trials were given respectively.

Day 12. The animal was now presented with two buttons L_1 and L_2 at each trial, and trained to press the button not initially preferred. Fifty trials were given.

Day 13. This position habit was continued for a further fifty trials.

Day 14. The other button was now made the "correct" one and fifty trials again given.

It is the data obtained from these fifty trials of day 14 which are analysed below. This particular day was chosen because it was felt that a training period covering the complete transition from one habit to another would be the most suitable for a stochastic analysis.

Results

The results of day 14 are given in columns 1 and 2 of Table I. The response times are reduced by a constant factor of 0·7 seconds which was considered to be the minimum response time possible.

Using the choice sequence of the animal, the estimates of ρ and α are 0·15 and 0·18 respectively. Using the response times, $r_1 - 0·013$, $w_1 = 0·086$ and $a = 0·021$, so that $\rho' = \dfrac{r_1}{r_1 + w_1} = 0·13$ and $\alpha' = \dfrac{a}{r_1 + w_1} = 0·21$. Thus the three parameters

r_1, w_1 and a, appear to constitute an adequate description of the data.   In Figure 2, the "theoretical" and observed cumulative number of successes at each trial are plotted together and indicates no gross differences between them.   The theoretical curve is conditional to the previous experience of the subject.   Mathematically expressed the ordinate of this curve at the nth trial is $\sum\limits_{t=1}^{n} P(t/k_t)$.

TABLE I

LEARNING DATA AND THEORETICAL MEAN LATENCIES, M_t

Trial No., t	Response X_t	Latency T_t	M_t	Trial No., t	Response X_t	Latency T_t	M_t
1	0	5·80	10·00	26	0	3·30	3·57
2	1	2·30	10·00	27	0	22·80	"
3	0	11·05	8·47	28	1	2·30	"
4	0	2·80	"	29	0	2·80	3·36
5	0	2·80	"	30	1	0·30	"
6	0	8·55	"	31	1	5·80	3·16
7	0	4·55	"	32	1	0·55	3·00
8	0	14·55	"	33	1	1·55	2·84
9	0	3·05	"	34	1	0·80	2·70
10	0	41·30	"	35	1	1·05	2·58
11	0	6·05	"	36	0	0·55	2·46
12	0	1·30	"	37	1	1·80	"
13	1	3·80	"	38	1	0·55	2·36
14	1	9·05	7·35	39	1	0·80	2·26
15	0	11·80	6·49	40	1	1·05	2·17
16	1	2·05	"	41	1	0·55	2·09
17	0	14·30	5·81	42	1	0·05	2·02
18	1	2·30	"	43	1	1·05	1·94
19	1	4·05	5·26	44	1	1·05	1·88
20	1	8·80	4·81	45	1	4·30	1·82
21	0	2·30	4·42	46	1	0·55	1·76
22	1	2·55	"	47	1	2·30	1·71
23	1	5·05	4·10	48	0	1·30	1·66
24	1	0·80	3·82	49	1	1·55	"
25	0	4·05	3·57	50	1	0·05	1·61

Theoretical mean times are given in column 3 of Table I.   In order to assess the adequacy of the description of response times, the frequency distribution of e^{-R_t}, where $R_t = \dfrac{T_t}{M_t}$ is given in Table II. This distribution should, in theory, be rectangular in the region (0,1), with a mean of 0·5.   The mean of the ungrouped response times is 0·53 which does not differ significantly from the theoretical mean latency transform. As a final check on the theoretical mean latencies, e^{-R_t} has been plotted against the trial number, t, in Figure 3.   Apart from a rise in the values in trials 30 to 40, there is no indication of any special changes in the distribution of the latency transforms as learning progresses.   As the relative simplicity of the descriptive process may have appeared to have been lost in a welter of equations, a graph of the hypothetical rates of responding, from which the stochastic description can be entirely derived, is given in Figure 4.
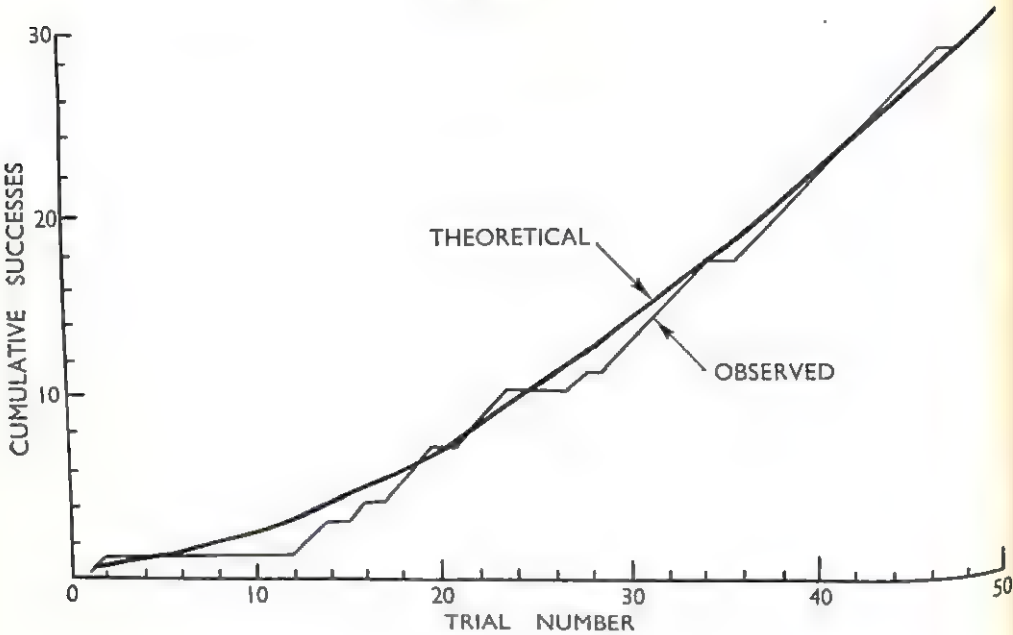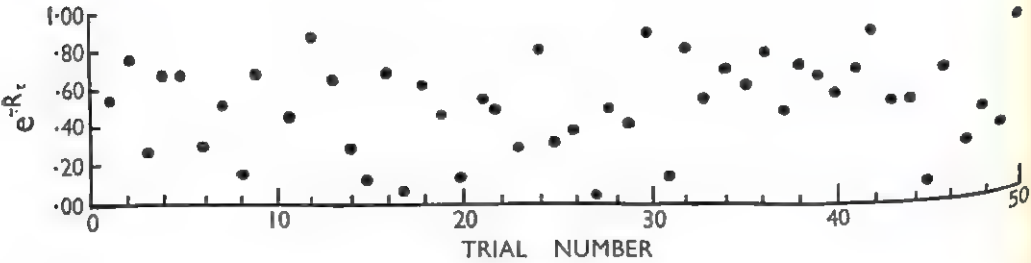
FIGURE 2

TABLE II

THEORETICAL AND OBSERVED DISTRIBUTION OF THE LATENCY TRANSFORMS, e^{-R_t}

e^{-R_t}	0·00–0·10	0·10–0·20	0·20–0·30	0·30–0·40	0·40–0·50	0·50–0·60	0·60–0·70	0·70–0·80	0·80–0·90	0·90–1·00
Observed frequency	4	4	4	4	5	8	5	9	4	3
Theoretical frequency	5	5	5	5	5	5	5	5	5	5

FIGURE 3

The intention of this paper is to illustrate a method for describing individual learning data and no claims are made for the particular theoretical context of the stochastic description employed. However, in order to show that the results are not entirely unique to one animal a brief report on the results of a further subject, run under identical conditions to the first, are now given. The parameter estimates based on the choice sequence were $\rho = 0·19$ $\alpha = 0·06$. The latency data gave

$r_1 = 0.023$, $w_1 = 0.108$, $a = 0.009$, hence $\rho' = 0.18$ and $\alpha' = 0.07$. A comparison of the theoretical and observed cumulative responses is given in Figure 5.
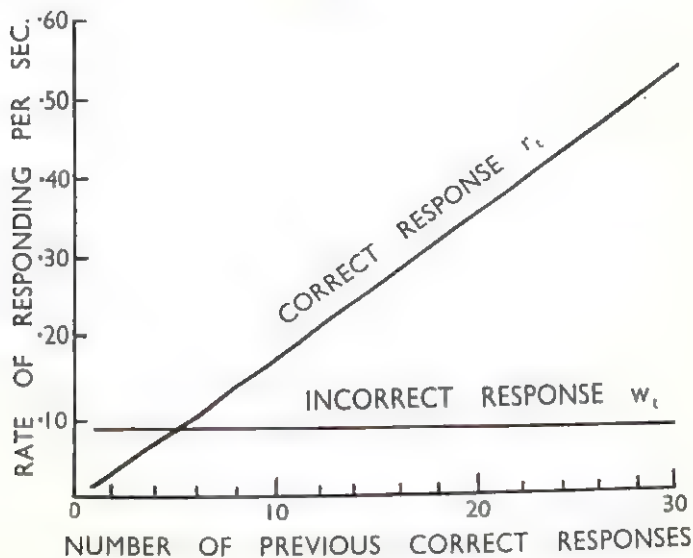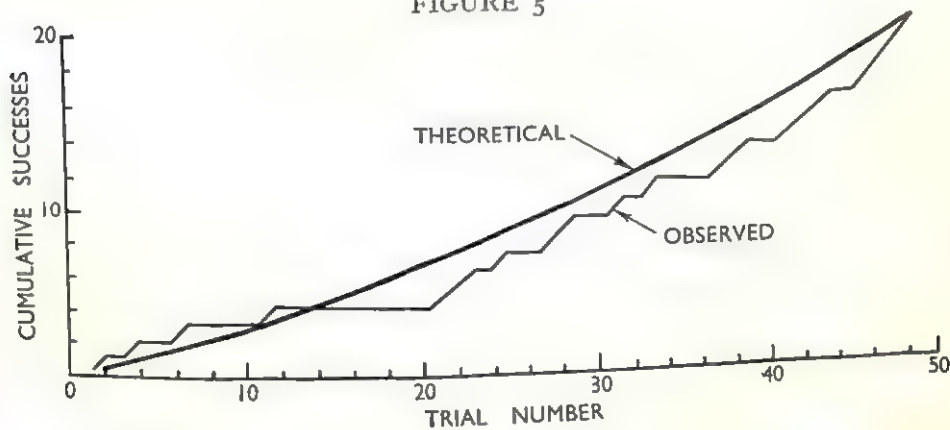
FIGURE 4

FIGURE 5

DISCUSSION

There can surely be no doubt that a theoretical description applicable to the learning behaviour of an individual subject has many advantages. One of the problems in the study of learning is to find an appropriate index of learning by which behaviour under different experimental conditions may be usefully compared. A stochastic description of the kind presented in this paper permits this comparison to be made in terms which are more easily given theoretical and empirical referents than the more generally employed indices, such as the number of trials to reach a given arbitrary criterion of learning. Furthermore, the determination of relations between various measures of learning becomes a more feasible proposition.

The stochastic process employed in the present paper is, of course, a very elementary one. In particular the effect of non-reward upon behaviour has been entirely ignored and the very simplest assumption about the hypothetical response rates, namely that they follow a Poisson process, has been selected. It is not claimed that either of these assumptions is justified by the results of the experiment. Many more observations and improvements in techniques for assessing goodness of fit and of determining the relation between the various kinds of parameter estimates are obviously required for this purpose. Nevertheless, the results are sufficiently promising to suggest that the problems of describing the learning behaviour of an individual subject, and of examining the relation between different aspects of learning behaviour, can be offered a practical solution by means of a stochastic process.

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THE EFFECT OF INTERPOLATED EXPERIENCE ON VISUAL RECOGNITION

BY

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Two hundred twelve-year-old children were shown a circle-with-gap figure and, five minutes later, required to recognize this figure from among a graded series of similar figures. The interpolation of circles with different sizes of gap resulted in retroactive interference, the recognition error being either positive or negative according to the nature of the interpolated figures. The results also draw attention to the dependence of certain recognition errors on the age of the subject and indicate a developmental trend in the perceiving and recognizing of simple figures.

I

INTRODUCTION

If someone is shown a visual figure and later recognizes it by picking it out from an array of figures, his recognizing, like his remembering in general, is liable to error. It is of interest to discover the sources of this error and the sources which have been suggested are basically three in number. The first is perceptual: the figure may be incorrectly perceived in the first instance. This source of error is particularly evident where the figure is of the complex, meaningful variety since it is here that the interpretative, assimilative nature of perceiving is most conspicuous and that perceiving can be most readily influenced by experimentally induced sets and attitudes of various kinds. The second is mnemonic: experiences interpolated between the time of perceiving and the time of recognizing may distort what is recognized. This is, of course, the phenomenon of 'retroactive' interference which has been so extensively investigated in the remembering of nonsense syllables. The third suggested source of error is also mnemonic. Proposed by Gestalt theory, it is held to involve spontaneous and dynamic activity within the 'memory trace.'

Hebb and Foord (1945) sought evidence either for or against the existence of this third elusive process of autonomous change. Their investigation was carefully designed to avoid those methodological flaws from which earlier studies had suffered (see Osgood, 1953, pp. 587–93, for a summary of the issue). Subjects were shown a single circle-with-gap figure and later required to recognize this figure when contained in a series of circles with gaps of varying width. Recognizing was tested at five minutes and at twenty-four hours after the original presentation and different groups of subjects were used for each time interval, i.e., any one subject was required to recognize the figure only once. Results completely failed to support the Gestalt theory. George (1952) replicated this experiment using time intervals of five minutes, twenty-four hours, four days, and eight days. Again, there was no evidence for progressive changes in recognizing with increasing lapse of time. In short, the data of these investigations showed the occurrence of recognition errors which were at a maximum in five minutes or less and then became relatively static for an undetermined period of greater than eight days. It is important to realise that the question at issue is not whether recognition error occurs but whether such error shows progressive change with lapse of time. The critical comparison is between remembering at one time and remembering at some later time under conditions where there are

no interfering experiences between the time of perceiving and of remembering, i.e., where it is different groups of subjects and not the same subjects who remember on each occasion.   It is this comparison which leads to the conclusion that there is no systematic difference (e.g., a tendency to closure) between earlier and later remembering.   Comparison between what is remembered and what was originally shown to the subject is another matter.   Here there may be a systematic difference (see below). Hebb and Foord would presumably agree with Osgood (p. 590) and others in ascribing this systematic error to perceptual rather than to mnemonic processes, i.e., to processes occurring during, or a few seconds after, the physical presence of the stimulus rather than some minutes or longer after the removal of the stimulus.   It is the existence of the latter, long-term change which is denied by Hebb and Foord's data not that of the former, more or less immediate change.   Why the same stimulus should be perceived differently on the second presentation is a problem to which no satisfactory solution has yet been discovered although it is a problem which is as old as the time error found by psychophysics in studying the process of successive comparison.

The conclusion would seem to be that there is only one basic source of mnemonic (i.e., relatively long-term) distortion, namely, interference.   The present investigation seeks to study this source and, to this end, the circle-with-gap material of Hebb and Foord is used.

II

Experiment

Subjects and materials

The subjects were 200 (108 girls and 92 boys) children aged between 12 years 0 months and 12 years 11 months.   The material was identical with that used by Hebb and Foord. It comprised twenty-four circles with gaps which increased, by steps of $2\frac{1}{2}°$, from a gap of $2\frac{1}{2}°$ (figure 1) to 60° (figure 24).   These were drawn with indian ink on separate 7·5 cm. squares of white cardboard, each circle being 5·6 cm. in diameter.   Numbers 8 and 16 of the series (20° and 40° gaps) were duplicated on identical pieces of card.   These were for use as the stimulus figures, one of which each subject would see and be asked to remember.   The whole series of twenty-four circles constituted the recognition series.   Also duplicated were two series of circles which were to act as the interference series.   The first ('small') series comprised numbers 1 to 6 inclusive (i.e., $2\frac{1}{2}°$ to 15° gaps by intervals of $2\frac{1}{2}°$).   The second ('large') interference series comprised numbers 19 to 24 inclusive (i.e., $47\frac{1}{2}°$ to 60° gaps by intervals of $2\frac{1}{2}°$).

Procedure

There were three phases.   (1) Presentation of the stimulus figure.   Each subject was tested individually in a quiet room of the school attended by the children.   When rapport had been established and the subject's name noted, instructions were given as follows.   "I am going to show you a card.   I want you to look at it very carefully and to try to remember what is on it.   Then later on, we'll see how well you *do* remember it. All right?"   The stimulus card was then handed to the subject who was allowed to examine it for exactly five seconds.   After this time, the experimenter said "Fine" and made a gesture so that the card was handed back.   (2) Presentation of the interference series.   Immediately following the removal of the stimulus figure, the experimenter announced: "Now I am going to show you some more cards.   But this time I want you to tell me which of the gaps in these circles is the smaller."   While giving these instructions, the experimenter presented two of the cards from the interference series of six.   When the subject had indicated which gap was smaller, another two cards from the same series was likewise presented.   In this way, the subject was given fifteen different pairs of cards representing the total number of pairs possible using the six cards of the interference series.   These pairs were presented in randomised order.   Half of the subjects were asked to indicate the smaller of each pair and half to indicate the larger.   After giving a word of praise for all performances irrespective of correctness, the experimenter went on to note date of birth, school class, and sex of the subject.   Until the standard five-minute

period was completed, subject and experimenter conversed on any topic of interest, usually the impending Christmas vacation. (3) Recognizing the stimulus figure. When five minutes had elapsed since the presentation of the stimulus figure, the experimenter said: "Fine. Now remember the card I showed you at the beginning? Well, I am going to show you a pile of cards and I want you to pick out the card I showed you at the beginning. All right? Go ahead." The subject was presented with a pack containing the twenty-four cards of the recognition series and allowed to go through these cards as often as he wished until he decided on the figure which was the same as that originally shown. The pack was presented to all subjects in the same cyclical order but the top card was varied for each subject in random fashion. Thus the recognition series was interrupted at any possible point thereby avoiding any consent error due to having, say, number 1 always on top. The subject's choice was recorded by noting the number of the figure selected.

At each of three stages in the procedure, there were two alternative conditions. Thus, the stimulus figure could be either 40° or 20°: the interference series could be drawn either from the six largest- or the six smallest-gapped figures of the total series: and the instructions could be either to find the larger member of the interference pairs or the smaller member. In combination, these alternatives gave rise to eight different treatments. Accordingly, each subject was randomly allocated to one of eight different groups. Each group contained twenty-five subjects and was treated as specified in the first three columns of Table I. In this table, specific figures are referred to simply in terms of the number of degrees of gap contained.

III

RESULTS

The recognition errors of each of the eight groups of subjects are summarized in the last two columns of Table I. The second last column shows the mean recognition error (in degrees) for each group: a plus error indicates that the recognized figure had a larger gap than had the stimulus figure while minus indicates that the gap was smaller than in the stimulus. The last column shows the standard error of each preceding mean.

TABLE I
PROCEDURE AND RESULTS

Group	Stimulus figure	Interference series	Instructions	Mean error	S.E. of mean error
I	40°	$47\frac{1}{2}$–60°	Larger	+ 5·5°	1·615
II	40°	$47\frac{1}{2}$–60°	Smaller	+ 6·7°	2·186
III	40°	$2\frac{1}{2}$–15°	Larger	− 3·7°	3·309
IV	40°	$2\frac{1}{2}$–15°	Smaller	− 6·0°	2·738
V	20°	$47\frac{1}{2}$–60°	Larger	+ 7·8°	3·065
VI	20°	$47\frac{1}{2}$–60°	Smaller	+20·8°	3·203
VII	20°	$2\frac{1}{2}$–15°	Larger	− 0·5°	1·445
VIII	20°	$2\frac{1}{2}$–15°	Smaller	+ 0·7°	1·375

To discover whether the eight different treatments produced any difference in the size and direction of the recognition error, the data were submitted to analysis of variance. The outcome of this analysis showed that the different treatments did indeed affect the recognition error. A further analysis of variance was undertaken to determine the specific sources of variation and the results of this three-by-three analysis are given in Table II. (Throughout this analysis, errors were treated in terms of $2\frac{1}{2}$° units or steps.) Four conclusions can be drawn. First, there was variance due to the stimulus figure used. With the 20° stimulus, the mean recognition error (100 subjects) was +4·73°: with the 40° stimulus, the mean recognition

TABLE II

ANALYSIS OF VARIANCE

Source	Sum of squares	Degrees of freedom	Estimate of variance	F-ratio	Level of signif.
1. Stimulus	134·48	1	134·48	6·91	0·01
2. Interference	811·08	1	811·08	41·67	0·001
3. Instructions	4·50	1	4·50	—	not sig.
Interaction 1 × 2	5·78	1	5·78	—	not sig.
Interaction 1 × 3	13·52	1	13·52	—	not sig.
Interaction 2 × 3	13·52	1	13·52	—	not sig.
Interaction 1 × 2 × 3	1·62	1	1·62	—	not sig.
General effect	228·98	1	228·98	11·76	0·001
Error	3737·52	192	19·466		
Total	4956	200			

error (100 subjects) was +0·63°. The difference between those two means was significant at the 0·01 level of probability demonstrating that, other things equal, the opening effect with the 20° stimulus was greater than with the 40° stimulus. Second, there was variance due to the nature of the interference series. With the series selected from the six smallest-gapped figures, the mean recognition error (100 subjects) was −2·38°. With the 'large' interference series, the mean recognition error (100 subjects) was +7·73°. The difference between these means was significant at the 0·001 level. Thus, other things equal, when the 'large' interference series was used as opposed to the 'small,' it produced an opening as opposed to a closing effect. Third, there was variance due to a 'general effect.' In the design of the experiment, this source of variance was additional to the variables or the interactions of these variables. It represented a constant error of recognition taking all subjects into account. The mean error for all subjects was +2·68° and deviated from the expected mean error of zero by an amount which was significant at the 0·001 level. Thus, over the experiment as a whole, there was a significant tendency for the recognized figure to have a larger gap than had the stimulus figure. Fourth and last, there were no significant effects on recognition due to the two different forms of verbal instruction given in presenting the interference series.

IV

DISCUSSION

The chief importance of these results is that the recognizing of a simple visual figure can be systematically influenced by interpolating visual experiences of similar figures. The fourth conclusion is that the two different instructions given during the presentation of the interference series have no significant effect on the recognition error. This suggests that, with the material used, interference derives exclusively from interpolated visual rather than verbal activities and confirms the argument of Hebb and Foord that: "The circle-with-gap series was qualitatively continuous throughout, the difference between any two successive items being definable in words only by making an estimate of the linear or angular extent of the gap" (p. 342). Words, whether given spontaneously by the subject or interpolated by the experimental conditions, seem neither to assist nor to hinder accurate recognizing. The finding that interpolated visual experiences interfere with visual recognizing confirms

the conclusion of Zangwill (1938). It also points up the complicating effects of procedures involving the same subject in repeated remembering of a figure (see Hebb and Foord; also Hanawalt, 1952). Each remembering performance, whether it be recall or recognition, provides an interpolated experience which interferes with later remembering so that a constant remembering error, say an overestimation, becomes cumulative and progressively accentuated. Early experiments using repeated remembering of a visual figure found that what is remembered may undergo progressive and directed distortion. And it was on the basis of this finding that Gestalt theory originally (and it would seem wrongly) postulated the existence of autonomous changes in the 'memory trace.'

The operation of interference of a somewhat different kind is indicated by the first conclusion drawn from the present experiment. Other things equal, the opening effect is greater for the 20° stimulus figure than for the 40° stimulus. It may be that this difference is due to some configurational property of the two figures in question. But it seems more than probable that the difference reflects, rather, the well-known 'central tendency of judgment', that is, the tendency for judgments to be displaced from the extremes towards the centre where each stimulus judged forms part of a graded series of stimuli. In this experiment, the subject recognizes by selecting the stimulus figure from a series of twenty-four figures. The 20° figure is number 8 of the series and the 40° figure is number 16. Taking into account the fact that both stimulus figures show a positive recognition error and assuming, for the moment, that the amount of this error is equal for both figures, how would this error be affected by the operation of a 'central judgment tendency'? The recognized figure would be biased towards the central or 'neutral' figure of the recognition series. Thus, the positive error in recognizing figure 8 would be increased while the positive error in recognizing figure 16 would be decreased. The final outcome would be that the 20° figure would have a larger positive recognition error than the 40° figure—which is exactly what was found. In putting forward an account in terms of a 'central tendency of judgment,' all that is really being done is to assert once more the effect of interpolated experience on recognizing. It is noteworthy that this account is open to experimental test. All that need be done is to vary the 'neutral point' of the recognition series relative to the stimulus figure. Such systematic variation should produce systematic change in the recognition error, quite independently of which specific figure is used as the stimulus. Furthermore, it is a corollary of this prediction that the error will vary according to the way in which the subject sets about his recognizing task. If he starts with the smallest-gapped figure of the recognition series and proceeds to look at figures with progressively larger gaps until he comes to (or even goes a little beyond) the stimulus figure, his error will be more negative than if he proceeds similarly from the other end of the recognition series.

The larger positive recognition error in recognizing the 20° as opposed to the 40° stimulus figure is an effect which depends on the age of the subjects examined. The effect is strong in 12-year-olds as shown by the present experiment and by a statistical analysis of the data given by Hebb and Foord (p. 345) for their Junior High School subjects. Yet, in adults, the effect is absent as shown by the results obtained from adult subjects by both Hebb and Foord and by George. The suggestion is that adults are less susceptible to the 'central judgment tendency' than are children. (A similar suggestion emerges from the experiments of Lambercier (1946) on size constancy.) It is tempting to propose that the findings both of Lambercier and of Hebb and Foord represent yet another instance of the broad developmental trend towards increasing 'perceptual articulation' (see Hunter, 1954). A large number of studies, varying all the way from psychophysics to Rorschach, reveal that the younger

subject perceives more in terms of the undifferentiated whole characteristics of the situation and is less able to abstract parts from this whole. In the Hebb-Foord situation, the single figure to be recognized might be regarded as a part while the entire recognition series is the whole. Expressed in this way, it is plausible to suppose that the present age difference reflects a widespread difference in the subjects' perceptual characteristics. Now if, as argued above, the 'central tendency' in this experiment is due to interference from the figures of the recognition series, this suggestion also implies that adults might well be less susceptible to retroactive interference in general, that their disparate experiences are more 'self-contained.' If this were so and if adults were run through the experiment reported—with the same material and the same stimulus exposure time—they would show themselves less influenced than were the children by interference from the interpolated figures. All this is, however, speculative and awaits confirmation from experiments explicitly designed to study developmental aspects of retroactive interference in general and of 'central tendency of judgment' in particular.

Another developmental difference which may be closely related to the above is found in considering the third finding of the present experiment. This finding, that there exists a significant over-all opening effect in recognizing, confirms what has already been found by Hebb and Foord. Once again, this effect seems to depend on the subjects' age. With children as subjects, Hebb and Foord found that, after five minutes, the 20° figure group had a mean recognition error of $+8.88°$ and the 40° figure group had a mean error of $+2.58°$. Both of these means differed from zero at the 0.01 level. In contrast, the adult groups showed mean errors which did not differ significantly from zero. Likewise, George's adult subjects showed no constant recognition error, nor did the adult subjects of Hanawalt (1952) in recognizing, by the method of equal comparison, a circle with an 18° gap. Thus, it may be concluded that the recognizing of a circle-with-gap figure involves a constant error provided the subjects are between 12 and 15 years but that, with adult subjects, no such constant error appears. Unfortunately, there is no information on the recognition errors of children beneath the age of 12 years.

There still remains the problem of explaining the children's recognition error in terms of perceptual processes. As stated in the introduction, this is a problem which has not yet been solved. It is also a problem on which the experiments considered in this paper can throw no light. Unlike this problem, however, the problem of why children show this positive recognition error while adults do not is one about which an, at least tentative, hypothesis may be proposed. Since the early demonstrations of the Gestalt psychologists, there can be no doubt that visual figures have configurational or 'field' properties. There has also been some suggestion that these configurational properties dominate the perceiving of children more than they do that of adults. The evidence for this suggestion is by no means unambiguous. Thus, in his much quoted experiment, Heiss (1932) had subjects, aged 3 to 19 years, select a particular wooden block from a number of differently shaped blocks which were (a) scattered in an unorganized group, and (b) organized into a closed pattern. At all age levels, selection from the 'patterned' array required a longer time than from the 'scattered' array but there was a greater discrepancy between these times at the younger than at the older age levels. This supposedly demonstrated that the task of abstracting from a 'gestalt' is relatively more difficult for the younger subject, i.e., that the dominance of the configurational whole decreases with increase in age. However, there are so many dubious points about Heiss' procedure that one of the senior author's students (R. B. Bett) repeated the experiment with modified material and under improved experimental conditions. Bett used 100 subjects aged between

7 and 15 years and confirmed that, with increasing age, the selection time progressively diminished. But, with the particular material and age range studied, there was no indication whatever that it was easier to select from the 'scattered' than from the 'patterned' array. However, despite the ambiguity of much of the work on ontogenesis of perceiving, there is an impressive kernel of evidence to support the main contention. Not the least of this comes from the study of those geometrical illusions where the perceiving of some part is distorted by the perceived form of the whole figure. Thus, Piaget (1942) studied the distorting effect of the outer circle in the Delboeuf illusion with subjects aged 5, 7, 8, and 10 years and also with adults and found that the degree of distortion decreased progressively with increasing age. Likewise, Piaget and von Albertini (1950) found that the Müller-Lyer illusion was greater for children between 5 and 9 years than for adults. Such findings indicate that adults are more able to react to isolated parts of a complex figure and resist the configurational 'tensions' of the figure as a whole. It seems not unreasonable to extrapolate this indication and propose that the positive recognition error of Hebb and Foord's younger subjects reflects their greater dependence on configurational characteristics of the circle-with-gap figure. This proposal is similar to that made in discussing the younger subjects' greater positive error for the 20° as opposed to the 40° stimulus figure and the two proposals might be brought together in a single statement as follows. In regard to the circle-with-gap figure, the difference between the recognition errors of the children and of the adults reflects a difference in the articulateness of their perceiving, that is, a difference between organization dominated by diffuse, configurational properties of the whole and organization in terms of discrete but interrelated parts. This statement is, of course, tentative and relatively vague. But it attempts to provide a working hypothesis about age differences in recognition error which, so far, seem difficult to assimilate to contemporary psychological knowledge in any other way.

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SENSORY FACTORS IN THE VOLUNTARY APPLICATION OF PRESSURE

BY

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The experiments described attempt to assess the extent of sensory control of voluntary movements of the forearm and finger when the contractions of the muscles concerned are approximately isometric.

In the first two experiments, records were obtained of the accuracy with which subjects were able to reproduce a particular pressure with the ulnar border of the hand at different pressure values and rates of application. Results show a statistically significant increase in error of reproduction of pressure as the pressure applied decreases and the rate of application is increased.

In the third experiment, using an applied force of 25 gms. weight, a significant difference in accuracy of reproduction of finger pressure between the maximum and a slower rate of application is also shown, but not when the digital nerves are blocked by a local anæsthetic. Furthermore, there is no significant difference in accuracy of reproduction of finger pressure between the anæsthetized and unanæsthetized condition at the fastest rate of application. On the other hand, at the slow rate of application, there is an increasingly significant difference between the anæsthetized and unanæsthetized condition as the applied force is reduced below 400 gms. weight, in favour of the control condition.

In the fourth experiment, subjects were trained to apply a force of 25 gms. weight using a small area of skin on the palmar surface of the index finger, then tested for their ability to reproduce the same force before and after changing the size of the area of application. Results show a marked change in applied force corresponding to the change in size of skin area used.

It is concluded that below an applied force of about 400 gms. weight, the maximum accuracy in reproduction of finger pressure is dependent upon cutaneous receptors.

The results of these experiments are discussed in relation to evidence from other experimental, clinical and animal studies and it is suggested that the major function of afferent impulses from a limb in the initiation of voluntary movements is probably facilitatory rather than sensory.

INTRODUCTION

Several studies have been carried out to determine the source of sensation in the appreciation of passive movement in man (Goldscheider, 1889; Pillsbury, 1901; Winter, 1912; Meyer, 1921; Browne, Lee and Ring, 1954), but very few experiments have been designed to investigate the rôle of the various receptors in active movement.

Probably the first experiments in this respect were carried out by Weber (1846) who attempted to determine the extent to which touch and muscular effort are involved in the discrimination of objects by weight. He found that when the subject was blindfolded and two weights were placed in succession on the two distal phalanges of two or three fingers with the hand placed flat on the table, weights of a ratio $14\frac{1}{2}$: 15 or 29 : 30 could with difficulty be distinguished. In the second experiment, a similar procedure was used except that the subject actually lifted each weight by means of a cloth. In this instance, two weights of a ratio of 39 : 40 could be distinguished. However, although the first experiment was probably confined to stimulation of cutaneous receptors, the second situation could have involved receptors in several joints as well as muscles, so that it is not possible to assign the difference in discrimination to a particular sensory source.

At a much later date, von Frey (1926) attempted to assess the accuracy of dupli-
cating a particular extent of movement in flexing the finger under various conditions
and concluded that the receptors mediating the sensation of movement lie in the
tissues surrounding the muscles and tendons concerned. However, owing to the
limitations of the method of recording, some of the variables in his experiments were
not particularly well-controlled, so that his conclusions are open to considerable doubt.

More recently, Gibbs (1954) compared the efficiency of a free-moving control with
a control having strong spring-centreing in a "tracking" situation and the apparent
superiority of the pressure control he attributed to a difference in feedback of sensory
information in the two conditions. On the other hand, Weiss (1954) made a similar
comparison between pressure and displacement controls, using a single reponse task
without visual guidance and found that movement cues were more important than
pressure in such a task. However, it seems likely that the conflicting results from
these two studies are due to the very different experimental conditions used, while
neither investigator appears to have made much attempt if any, to control the actual
movements involved in terms of joints and muscles or the skin area used in the
application of pressure.

Clinical evidence on the subject is also somewhat confused, but the accurate
observations of Duchenne (1872) point to the joints as the most sensitive elements in
the judgement of movement. From clinical observations and the results of stimulation
experiments, he concludes that while a dull painless or painful sensation may be
derived from muscle itself, knowledge of the attitude and movement of the limbs is
mainly due to articular sensibility.

In monkeys, Mott and Sherrington (1895) and Twitchell (1954) have shown by
their de-afferentation experiments that deterioration of limb movement is much
greater after severing the afferents from skin than from muscle, while complete
de-afferentation renders a limb far more deficient in voluntary movement than
ablation of the corresponding part of the motor cortex. This finding has also been
confirmed by Lassek (1955).

The present study was undertaken to determine the contribution of different
receptors to the control of voluntary movement in man when the contractions of the
muscles concerned are approximately isometric. Such conditions were chosen in
order to simplify the experimental situation by reducing the variables to be controlled
to a minimum. For example, in the isometric contraction of muscles acting about a
joint well within its normal range of movement, it was considered that stimulation of
receptors in joints and ligaments would be negligible, leaving only the possibility of
receptors in skin, muscle and tendon to provide information on the state of the limb
concerned. Actual displacement of the limb was prevented as far as possible by
using a strain-gauge apparatus to record the force applied.

The first two experiments were designed to investigate the accuracy with which
subjects were able to reproduce a particular pressure with the hand at different
pressure values and rates of application, while the third and fourth experiments were
planned to determine the source of sensory control of the application of pressure at
given values of applied force and application rate.

A preliminary account of part of this work has already been given (Provins, 1955).

APPARATUS

The experimental apparatus has been described previously (Provins, 1956) and consists
of a spring-steel bar on each side of which is mounted a strain-gauge element. One end of
the spring-steel bar is held rigid in an adjustable frame while to the other end is attached a
brass disc against which the subject applies the pressure required. The strain gauges form

part of a Wheatstone Bridge circuit suitably coupled to a pen-recorder so that the deflection of the pen is directly proportional to the force applied at the brass disc.

At the beginning of each experimental session, the apparatus was calibrated by turning the spring-steel bar horizontal, and placing on the brass disc, a weight equivalent to the deflecting force used in the subsequent experiment. The recorder was then adjusted to give 25 mms. deflection of the pen. The calibration forces used are given in the description of the individual experiments. The actual displacement of the brass disc was directly proportional to the applied force and varied with the size of spring-steel bar used. Thus, in the first two experiments, movement of the free end due to the maximum force applied was 0·07 cms., while in the last two experiments it never exceeded 0·24 cms. The natural frequency of the thin bar was 21 cycles per second, while that of the thicker bar exceeded the response frequency of the recording apparatus which was flat from 0 80 cycles per second.

METHODS

The subject sat in a comfortable posture with the trunk upright and placed the hand in the required position. Two positions were used, the first with the upper arm firmly supported in a horizontal position and the elbow flexed to a right-angle so that the forearm was vertical with the hand uppermost. In this position, pressure was applied by an area on the ulnar border of the hand. In the second arm position, the elbow was flexed to a right-angle and the lower arm comfortably supported in a horizontal position so that pressure was applied by an area on the palmar surface of the distal phalanx of the index finger.

The subject was told that in every instance he was required to produce a 25 mm. pen deflection, although in some experiments the actual force corresponding to this deflection differed from one run to another. He was told to apply a continually increasing pressure to the brass disc at the rate required until he thought he had produced a 25 mm. deflection, then to relax and take his finger or hand away from the disc surface. Contractions were made at intervals of 15 seconds, and in each instance a preliminary warning was given to enable the subject to bring his hand or finger into position so that the skin area specified was not quite touching the brass disc. When he was told "now," he closed his eyes to eliminate the possibility of using visual aid and started applying the required pressure. In the experiments where knowledge of results was given, the subject was told the deflection achieved to the nearest 0·5 mm. after he had completed the contraction and returned to the relaxed position. Twenty observations were recorded in each run during the first experiment, but this was reduced to ten in the subsequent experiments.

First experiment. Ten healthy adult subjects, all members of the Department staff, attended for one session only. The accuracy with which each subject could reproduce a 25 mm. pen deflection at five different calibration values was recorded, one calibration value being used for each run. Pressure was applied by an area on the ulnar border of the preferred hand and each contraction was completed in the subject's own time, which was usually of the order of 2–3 seconds' duration. The subject was given knowledge of his results after each contraction and the pen deflection achieved was noted to enable the mean error for the run to be calculated later. The apparatus was calibrated before each run with force values of either 3,750 gms., 1,875 gms., 750 gms., 375 gms., or 187·5 gms. weight applied at the brass disc of radius 1·265 cms. Ten practice trials were given before recording the first run, and five practice trials before beginning subsequent runs. The order in which each subject completed the five runs was determined by a random order to minimize any training effect.

Second experiment. Six of the subjects previously used and four other members of the Department staff attended for one session only, when the accuracy with which each subject could apply the same pressure at five different rates of application was recorded. The apparatus was calibrated so that 375 gms. weight applied at the brass disc of 1·265 cms. radius produced a pen deflection of 25 mms. The subject was requested to apply the force in with the ulnar border of the preferred hand within certain time limits, but otherwise in the manner previously described. During ten preliminary trials or contractions, training was given in both the amount of pressure required and the rate of application. This practice enabled the subject to assume a surprisingly consistent rate before starting each recorded run, during which verbal reminder about the rate was rarely necessary. Instructions concerning the fastest run were merely that the application should be at the maximum

rate possible, and care was taken to examine the form of the deflection after each contraction, so that only sharply defined single spikes were accepted. The rates used in terms of time taken to complete the contraction were rate 1, < 0·25 seconds; rate 2, 0·5–1·0 seconds; rate 3, 1·25–2·0 seconds; rate 4, 2·5–4·0 seconds; rate 5, 5–7·5 seconds. The order in which each subject completed the five runs was randomized and knowledge of results given after each contraction, as in the first experiment.

Third experiment. Twelve medical students attended for two sessions, during which accuracy at five different force values using a gradual rate of application was tested. At the most sensitive calibration the effect of using the maximum rate of application was also tested. The arrangement of the two sessions was identical except that in one, the digital nerves of the subject's index finger were blocked with an injection of 6–8 ccs. 2 per cent. Xylocaine distal to the metacarpo-phalangeal joint. Sensation was tested immediately before and after this experimental session, which lasted about one hour, by using a blunted point of a pair of compasses. In three of the twelve subjects, sensation had begun to return when tested at the end of the session, and their results were discarded. In most of the subjects, therefore, 2 per cent. Xylocaine with 1 in 300,000 Adrenaline was given, and the block then lasted for about 3–4 hours, while in one instance absence of sensation was reported for as long as seven hours. The five calibration values used were 25 gms., 50 gms., 100 gms., 200 gms. and 400 gms. weight applied at the brass disc of 0·63 cms. radius to

TABLE I

SUMMARY OF EXPERIMENTAL CONDITIONS

Experiment No.	Subjects	Area	Duration	Calibration	Criterion of performance
1	10 Staff	Ulnar border 1·265 cms. radius	2–3 secs.	5 values compared: 3,750 gms. = 25 mms. 1,875 ,, = ,, 750 ,, = ,, 375 ,, = ,, 187·5 ,, = ,,	mean error
2	10 Staff	Ulnar border 1·265 cms. radius	5 rates compared <0·25 secs. 0·5 – 1·0 secs. 1·25–2·0 ,, 2·5 – 4·0 ,, 5 – 7·5 ,,	375 gms. = 25 mms.	mean error
3	(a) 9 students attended for two sessions in one of which the finger was anæsthetized (b) ,,	Palmar aspect distal phalanx index finger 0·63 cms. radius ,,	2–3 secs. 2 rates compared: < 0·25 secs. 2–3 ,,	5 values compared: 25 gms. = 25 mms. 50 ,, = ,, 100 ,, = ,, 200 ,, = ,, 400 ,, = ,, 25 gms. = 25 mms.	mean error ,,
4	8 students 2 staff	Palmar aspect distal phalanx index finger. two disc sizes: 0·63 cms. radius 0·315 ,, ,,	2–3 secs.	25 gms. = 25 mms.	transfer of training (mean pen deflection)

produce a pen deflection of 25 mms. Each contraction was completed in the subject's own time and was usually of the order of 2–3 seconds' duration except, of course, for the fast run. Subjects were given knowledge of their results after completion of each contraction at the same time as the pen deflection achieved was noted. Of the nine students used, six attended for the control session before the experimental session while the other three completed the series in the reverse order. The order in which the five force values were attempted by each subject in both sessions was according to a predetermined random order, including the comparison between the fast and slow rates of application at the most sensitive range. Six of the subjects completed these two runs before the "force" series, while the other three subjects completed the "force" series first.

Fourth experiment. Eight medical students and two members of the technical staff attended for one session only. The accuracy with which they could apply a particular force value at a gradual rate of application was tested in attempted flexion of the index finger. Subjects were trained to reproduce a 25 mm. pen deflection by pressing against a brass disc of a particular diameter in their own time which was usually of the order of 2–3 seconds duration. They were next tested under the same conditions, then with the disc area changed, and finally under the original conditions again. The two disc sizes used were of radius 0·63 cms. and 0·315 cms. Approximately ten contractions of preliminary practice were allowed before starting to record. The initial training run and three test runs comprised ten contractions each, with an interval of 15 seconds between the start of successive contractions both during and between runs. During the preliminary practice and the training run, subjects were given knowledge of results after each contraction, but in the subsequent three test runs, they had to rely entirely on their own judgement. Each subject completed two test series, in one of which his initial training and test was carried out with the larger disc, while in the other series he started with the smaller area. Half the subjects used the large-sized disc first and the other half first used the smaller disc area. Between the two series, a few minutes' rest was allowed, during which the subject was engaged in conversation on matters unrelated to the experiment. The apparatus was calibrated so that a force of 25 gms. weight applied to the brass disc produced a 25 mms. pen deflection.

The conditions of the four experiments are summarized in Table 1.

RESULTS

The difference between the actual pen deflection achieved at each contraction and the pen deflection desired (25 mms.) was obtained and the mean error calculated for each subject for each run in the first three experiments.

The mean error for the ten subjects applying pressure at the five different force values used in the first experiment is given in Table II, while the results of a comparison between the mean errors for the ten subjects at each calibration and the mean of the mean errors at all higher calibrations using the "t" test are also shown. It will be seen that over the range of values tested, the error increases as the force applied decreases and that the mean performance at each calibration differed significantly from the mean performance at all higher calibration values.

On the other hand, subjects who did well at one calibration value were not necessarily the best at any other level of applied force. This is indicated by the results of product-moment correlation tests between subjects' mean errors at the different calibration levels, which show that the value of r varies between $-$ 0·61 and $+$ 0·65 with an average value for the ten possible comparisons of $+$ 0·14.

Calculation of the mean actual pen deflections achieved (Table II) shows that at the lower calibration values there was a tendency to overshoot the target value of 25 mms. pen deflection and to undershoot at the higher calibrations.

The results of the second experiment are given in Table III, where it can be seen that at the fastest rate of application there is a substantial rise in the mean error recorded. The mean errors for the ten subjects at each rate of application were compared with the mean of the mean errors at all slower rate values by "t" test, and

TABLE II

COMPARISON OF MEAN ERRORS IN APPLYING PRESSURE AT
DIFFERENT FORCE VALUES

Force applied (gms.)	Mean pen deflection (mms.)	Mean error (mms.)	S.D.	D.†	S.E.*	Significance
187·5	27·2	6·03	1·67	2·68	0·512	P < 0·001
375	25·8	4·64	0·93	1·72	0·440	P < 0·01
750	24·8	3·62	0·88	1·06	0·228	P < 0·01
1,875	24·5	2·83	0·77	0·53	0·302	P < 0·2
3,750	24·1	2·30	0·76			

S.D. = Standard deviation of the mean error.
†D. = Difference between mean errors.
*S.E. = Standard error of the difference between mean errors.

the results show that as the duration of the contractions is reduced below approxi-
mately one second, the mean error increases significantly, particularly when the
contraction time is less than 0·25 seconds.

TABLE III

COMPARISON OF MEAN ERRORS IN APPLYING PRESSURE AT
DIFFERENT RATES OF APPLICATION

Duration of application (secs.)	Mean pen deflection (mms.)	Mean error (mms.)	S.D.	D.†	S.E.*	Significance
< 0·25	25·19	4·89	1·96	2·29	0·574	P < 0·01
0·5 – 1·0	25·24	3·11	1·71	0·69	0·253	P < 0·05
1·25 – 2·0	25·35	2·60	1·62	0·27	0·531	not sig.
2·5 – 4·0	24·86	2·21	1·42	0·24	0·290	not sig.
5 – 7·5	25·51	2·45	1·32			

S.D. = Standard deviation of the mean error.
†D. = Difference between mean errors.
*S.E. = Standard error of the difference between mean errors.

A comparison of the mean actual pen deflections for the ten subjects at the
different rate values shows no particular trend in this series, but a very slight tendency
to overshoot at nearly all rates of application as can be seen in Table III.

In this experiment, subjects also varied less in their relative ability between runs
as shown by tests of correlation between the mean errors for each subject at the
different rates of application, which gave an average value of r of $+ 0.57$ with a range
for the ten possible comparisons from $r = + 0.22$ to $r = + 0.84$.

The results of the third experiment are summarized in Table IV, which compares the mean error in the anæsthetized and control conditions. The mean error in both conditions increases as the pressure applied decreases as in the first experiment, but the error in the anæsthetized condition increases more rapidly. The differences between the mean errors for the nine subjects in the two conditions at corresponding values were tested for significance by "t" test, and the results are also given in Table IV. These differences show decreasing significance with increasing calibration value until at the heaviest calibration used (400 gms.), there is no significant difference in performance between the two conditions.

TABLE IV

EFFECT OF ANÆSTHETIZING THE FINGER ON THE MEAN ERROR IN APPLYING FINGER
PRESSURE AT DIFFERENT FORCE VALUES

	Experimental Series			Control Series			Comparison between Mean errors	
Force applied (gms.)	Mean pen deflection (mms.)	Mean error (mms.)	S.D.	Mean pen deflection (mms.)	Mean error (mms.)	S.D.	S.E.*	Significance
25	35·12	13·84	5·91	27·36	6·68	1·82	1·97	P < 0·01
50	30·29	8·59	3·48	26·39	4·69	2·14	1·66	P < 0·05
100	29·25	6·43	3·88	25·52	4·13	1·58	1·56	P < 0·2
200	26·28	4·62	3·16	25·29	3·00	0·84	1·14	P < 0·2
400	23·82	3·32	0·68	24·38	3·26	0·73	0·302	—

S.D. = Standard deviation of the mean error.
*S.E. = Standard error of the difference between mean errors.

The mean of the actual pen deflections recorded at each calibration value during the two series is also included in Table IV. There is a general tendency to overshoot at all except the maximum force values used in each series, while the extent of the overshooting markedly increases with decrease in calibration value, although this is more noticeable in the anæsthetized condition.

Tests of correlation between the mean errors scored by the nine subjects at each level of calibration gave low but mainly positive values of r in both series. In the control condition, values of r ranged from − 0·23 to + 0·64 with an average of + 0·17 and in the experimental series from − 0·17 to + 0·83 with an average of + 0·26.

The comparison between the fast and slow rates of application for both the anæsthetized and control series is shown in Table V, together with a comparison between the fast rates of application in the two series. Comparisons were made by "t" test between the mean errors for the nine subjects in each instance. It can be seen that the difference between the means for the fast and slow rates of application is not significant in the anæsthetized condition, but exceeds the I per cent. level of significance in the control experiment. A comparison of the errors at the fast rate of application between the anæsthetized and control condition shows no significant difference.

Comparisons were also made by "t" test between the means of the actual pen deflections for the nine subjects in each case to check on a possible change in the distribution of errors about the target value (25 mms.) which would not be apparent from the previous tests. The results are included in Table V and show no significant change in the mean deflections recorded in the different conditions.

TABLE V

EFFECT OF ANÆSTHETIZING THE FINGER ON THE MEAN ERROR AND MEAN PEN DEFLECTION ACHIEVED IN APPLYING FINGER PRESSURE AT TWO RATES OF APPLICATION

	Experimental Series		Control Series			
Rate	Mean error (mms.)	S.D.	Mean error (mms.)	S.D.	S.E.*	Significance
Slow	13·84	5·91			2·28	Not sig.
Fast	11·85	3·83			1·79	Not sig.
Fast			12·14	1·92	1·16	P < 0·01
Slow			6.68	1·82		

Rate	Mean pen deflection (mms.)	S.D.	Mean pen deflection (mms.)	S.D.	S.E.*	Significance
Slow	35·12	8·19			2·50	Not sig.
Fast	31·56	5·22			2·90	Not sig.
Fast			30·63	3·79	1·89	Not sig.
Slow			27·36	2.80		

S.D. = Standard deviation of the mean.
*S.E. = Standard error of the difference between mean values.

The mean pen deflection recorded for the ten subjects for each run in the two series of the last experiment is given in Table VI. The differences between the means of successive runs were tested for significance by "t" test and the results included in Table VI. It will be seen that the slight difference in mean pen deflection between the initial training and first test run before the disc area was changed, is not significant in either instance. However, changing the size of the disc from small to large and back again to small did cause a significant change in the mean pen deflection in the direction of maintenance of a constant pressure. A similar change of mean pen deflection in the appropriate direction with change in disc area was also recorded in the other test series, although only the change from small to large disc size was statistically significant. There also appears to be a progressive increase in the mean force applied independent of the size of disc used in both series.

Results of product-moment correlation tests between the four runs of experiment 4 for the ten subjects used in each series (Table 6) shows that in each case there was little correlation between the training and initial test run, but upon changing the disc area there was an increasing tendency for the differences between subjects to become stabilized as reflected in the increasing correlation values between runs.

The extent of individual differences in each experiment is indicated by the value of the standard deviation in each run (Tables II, III, IV, V and VI). Differences between successive contractions for each subject in any one run are not shown for reasons of space, but generally speaking, where the mean error for a run was low, differences between successive contractions were small, while a high mean error was usually associated with large fluctuations in applied force. This is indicated to some

TABLE VI

VARIATION IN MEAN FORCE APPLIED WITH CHANGE IN SIZE OF AREA
OF APPLICATION

	Disc size	Mean pen deflection (mms.)	S.D.	S.E.	Significance	Correlation value: r =
Order of series	Small	26·50	3·60			+ 0·22
				1·69	Not. sig.	
	Small	26·61	4·48			+ 0·52
				3·53	P ·. 0·01	
	Large	42·30	12·20			+ 0·76
				2·70	P < 0·02	
↓	Small	34·20	10·71			
Order of series	Large	28·32	2·67			− 0·08
				2·64	Not sig.	
	Large	30·28	7·25			+ 0·34
				2·72	Not sig.	
	Small	27·64	6·98			+ 0·75
				2·14	P < 0·05	
↓	Large	33·34	9·72			

S.D. = Standard deviation of the mean pen deflection.
S.E. = Standard error of the difference between mean pen deflections.

extent by the relatively small change in mean pen deflection from one run to another
in any particular experiment compared with the differences in mean error.

DISCUSSION

In both the first and third experiment at the lower values of applied force, there
was a tendency to overshoot the required pen deflection, while at the higher values
there was a tendency to undershoot, although the distribution of over- and under-
shoots was somewhat different in the two experiments (cf. Tables II and IV). The
effect evident in experiment 1 is similar to that noted by Jenkins (1947) and can
probably be explained in terms of a central tendency for the series where the range of
values tested falls within the range of forces which the hand and forearm may normally
be called upon to exert in everyday activities. The almost complete absence of such
a tendency in the data given by Weiss (1955) in his "pressure" series is somewhat
difficult to understand, although as he suggests, the discrimination of distance in his
experiments may have been so much finer than pressure discrimination that most of
the pressure information was rendered redundant. On the other hand, most of the
calibration values used in experiment 3 of the present series are probably less than
the forces usually exerted in finger flexion, so that the central tendency for this experi-
ment was no doubt modified by another series effect due to past experience (Wood-
worth and Schlosberg, 1955, p. 230). This probably accounts for the more marked
bias in the anæsthetized condition where current sensory information from the skin
was experimentally controlled. In the fourth experiment, after the initial training
run when knowledge of results was withdrawn, there was a general and progressive
tendency to overshoot the target value, similar to that noted by Macpherson, Dees
and Grindley (1948) in their experiments, and for which there was no apparent
explanation. However, in the present series, it seems probable that as the very low
calibration value of 25 gms. was used, the overall increase in pen deflection during

the experiment was again due to the influence of past experience, while changing the disc area between runs no doubt helped the process by tending to destroy the subjects' concept of the absolute amount of force to be applied.

The decrease in accuracy of application with decrease in the pressure applied at the relatively low pressure values used, agrees with the results obtained by Jenkins (1947) in a similar task and by many previous workers concerning judgements of stimulus intensity using different sensory modalities (Holway and Pratt, 1936; Woodworth and Scholsberg, 1955). These have included the accuracy of judging lifted weights which, of course, may entail quite a complex type of movement and the activation of receptors in many different kinds of structure. In such situations, however, the subject usually indicates his judgement verbally, whereas in the present series the accuracy of the pressure applied has been taken as a measure of his judgement. In other words, the results recorded in Table II may be an indication of the limitations of the motor side of the attempted movement as much as the sensory factors involved. This is also true, of course, for the control series of experiment 3, where the accuracy of application of finger pressure at different calibration values was examined.

However, the main purpose of the third experiment was to examine the effect of experimentally controlling the sensory information derived from different sources without interfering with the motor apparatus in any way. From a consideration of the origin and insertion of the various muscles involved, it seems most unlikely that the anæsthetic could have affected their activity as only the tendons of insertion pass distal to the metacarpo-phalangeal joint. The muscles mainly concerned with flexion of the index finger are in the forearm, namely, flexor digitorum sublimis and flexor digitorum profundus, and those concerned in extension of the index finger are extensor digitorum communis and extensor indicis proprius. Other muscles which may have contributed to the movement tested are the first lumbrical and the first dorsal and second palmar intcrossei, although there is experimental evidence that the lumbricals are not active during simple flexion at the metacarpo-phalangeal joint (Backhouse and Catton, 1954). That none of these muscles was affected by the digital nerve block is supported by the absence of any significant difference in performance between the control and experimental runs at the fast rate of application. In fact the mean error for the nerve block series at the maximum rate of application was slightly less than that for the control series (Table V). The impairment of performance in the anæsthetized condition at the slow rate of application, therefore, must be considered to be sensory in origin and to be due to the blocking of sensory sources which are normally relied upon in the judgement of the gradual application of pressure below approximately 400 gms. weight. The fact that the comparison between the normal and anæsthetized condition is also a comparison between sessions is, of course, a potential source of error. However, there is some evidence to show that in this type of task, while there may be considerable day-to-day fluctuations in performance, a general improvement with practice may be expected (Provins, unpublished). In this particular instance therefore, the results may be an underestimation of the effects of anæsthesia as six of the nine subjects completed the control session first.

Confirmation that at the most sensitive calibration value used, judgement of finger pressure at the slow rate of application is dependent on cutaneous receptors is given in the results of the fourth experiment. If the subjects had been relying on information from sensory endings in muscle or tendon, or any other constant source of sensory information, no change of applied force with change of disc area should have occurred, whereas a marked change of applied force did occur in the direction of maintaining a constant pressure. On the other hand, as the disc areas were in the ratio of 4 : 1, to

satisfy the equation $P = \dfrac{F}{A}$ (where P = pressure, F = force and A = area of applica-

tion), if the same pressure had been maintained, the force recorded should have changed in approximately the same ratio, whereas in fact, this was by no means the case. There are probably several factors which contribute to this. In the first instance, it seems likely that subjects attempted to compensate to a varying extent for the change in disc area, either deliberately or unwittingly. Certainly when they were asked for their comments at the end of the experiment, six of the ten subjects remarked on this particular point and four subjects definitely stated that they tried to . . . . "press less hard with the larger one." In other words, when the disc area was changed from, say, the small to the larger disc, subjects probably attempted to make the appropriate amount of contraction from memory, but the pressure perceived at about the force required was less than they had been trained to expect, so they applied a bit more. But it may be predicted that the "bit more," i.e., the increase in mean error on changing the disc area is unlikely to exceed the mean error in force applied when sensory information from the skin is completely blocked, i.e. of the order of 13·84 mms. pen deflection (Table V). Consequently the maximum mean pen deflection which may be expected on increasing the disc area is 25 + 13·84 or rather 26·61 + 13·84 = 40·45 mms., and the minimum on decreasing the disc area is 30·28 − 13·84 = 16·44 mms. The maximum deflection predicted is in surprisingly good agreement with the mean deflection recorded (42.30 mms.), considering the wide individual differences in this experiment and the fact that different subjects took part in experiments 3 and 4. On the other hand, it is rather more difficult to find an adequate explanation for the relatively small reduction in applied force with initial change of disc area to the small size disc. If to the predicted figure of 16·44 mms., another 2–4 mms. be added in respect of the general tendency to increase the applied force, there is still about 7 mms. deflection unaccounted for. It may be said, of course, that subjects compensated for the change in disc size more successfully in this instance, and that after all, the predicted figure is based on the maximum reduction in pen deflection which may be expected. But it still remains to be explained why the maximum should be achieved in one instance and not in the other. It seems probable that this is due to the fact that the amount by which the applied force can be reduced is extremely limited, while no such practical limitation applies in the opposite direction. Thus, in one series, the expected deflection is achieved, while in the other, the force applied is modified by the restricted range available.

The absence of any impairment of performance at the maximum rate of application of pressure in the anæsthetized condition in experiment 3 suggests that, unlike the slower rate used, the fastest rate of application is either (a) not subject to modification by sensory check or (b) graded by sensory information derived from the contraction of muscle itself, i.e., receptors in muscle or tendon. Apart from the fact that the experimental situation was designed to preclude adjustments during contraction by careful examination of the form of the pen deflection, previous evidence concerning the reaction time required for the amendment of a response in a comparable situation was found by Hick (1948) to be about 0·3 seconds, whereas the maximum time taken at the fastest rate of application in the present series was about 0·25 seconds. Similarly, in a task requiring continual correcting movements in relation to a visual display, Vince (1948) showed that, when the responses occupied up to 0·4 seconds, the level of error with the eyes open was the same as when the eyes were closed at the beginning of each movement, and that the accuracy of the movements deteriorated as the duration was reduced below 0·6 seconds. She therefore concluded that "the sole effect

of speed is to eliminate the opportunity of making adjustments based on sensory check." This explanation is equally applicable to the results of the present rate experiment, for as the duration of the movement increased, the error decreased rapidly at first, but as the time taken increased beyond about 0·5–1·0 seconds no significant improvement was obtained in the accuracy with which the pressure was applied—in this instance with the hand.

Spontaneous comments by the subjects on completion of experiments 2 and 3 emphasize the lack of sensory control during application at the maximum rate. The following comment is typical, "With the fast runs I was frustrated or disappointed at the result sometimes, when I knew that I had overshot but had no time to correct it." On the other hand, subjects were sometimes surprized to learn that they had overshot the mark when they thought they had undershot, and vice versa. This observation is in keeping with the findings of Fullerton and Cattell (1892). Other evidence supporting the conclusion that a graded movement of short duration may not be subject to sensory control is provided by Peters and Wenborne (1936), Taylor and Birmingham (1948) and Vince (1948).

If therefore the fastest rate of movement in these experiments is not subject to modification by sensory check whereas slower rates may be so modified, the absence of any significant difference in performance at the two different rates of application of finger pressure in the anæsthetized condition is somewhat unexpected. For at the slow rate of application, although the experimental situation was designed to deprive the subject of the possibility of obtaining adequate information from the skin, joints or ligaments of the finger, as the motor apparatus was unimpaired, for the same reasons, sensation derived from receptors in the muscles involved should not have been affected. It is also most unlikely that the anæsthetic blocked afferent impulses from the tendon receptors as these occur mainly at the tendino-muscular junction (Adams, Denny-Brown and Pearson, 1953). However, on completion of the experimental session when the subjects were asked to locate the source of sensation on which they based their judgements of applied force, there were no confident answers, and suggestions varied from vague feelings of strain in the palmar or dorsal region of the hand to similar sensations in the wrist and forearm or the metacarpo-phalangeal joint.

This lack of any improvement in performance in the anæesthetized condition at the slower rate of application cannot be explained in terms of adaptation rate of the receptors in muscle and tendon as in both structures they are relatively slowly adapting (Matthews, 1933). The results suggest therefore, that no sensory check was possible at the slower rate of application used because no adequate sensation was aroused.

It has previously been concluded by Lashley (1917) that the accurate movement of a single joint is possible in the absence of all excitation from the moving organs. This was based on a thorough examination of the direction and extent of active movements of the left knee in a patient who had received a gunshot injury to the spinal cord which produced a pattern of partial paralysis and anæsthesia of the lower limbs. These observations are supported by those of Strümpell (1902) concerning a man who had received stab wounds in the cervical region of the back, and as a result experienced complete anæsthesia of the right arm. The affected limb was capable of a considerable range of movements which were somewhat impaired when the patient closed his eyes, although he never misjudged direction.

On the other hand, evidence from de-afferentation experiments on animals (Mott and Sherrington, 1895; Lassek, 1953 (a); Lassek and Moyer, 1953; Twitchell,

1954) suggests that voluntary movement is all but impossible in the complete absence of afferent impulses from the limb concerned.

The indication is therefore, that while afferent impulses from a limb are necessary for the initiation of a voluntary movement, the sensory component, i.e., the conscious element, is not. In other words, a certain afferent inflow to the spinal cord appears to be necessary to facilitate voluntary movement of a particular limb irrespective of the degree of sensory information derived from it. The evidence from the present series of experiments supports one aspect of this view, while Gellhorn (1953) and Lassek (1953(b)) have previously stressed the facilitatory function of afferent impulses at various subcortical levels.

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THE ERROR POWER SPECTRUM AS A TECHNIQUE FOR ASSESSING THE PERFORMANCE OF THE HUMAN OPERATOR IN A SIMPLE TASK

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The measurement of performance of a human operator in a closed loop control system is considered; it is suggested that the power spectrum of the fluctuations of his tracking error (or error spectral density curve) gives a useful picture of performance and the equipment and technique for producing such a curve is described briefly.

As an example of the technique the power spectra obtained on several subjects performing a simple task with a pressure joystick are given and the implications of the shape of the curve are discussed.

I

INTRODUCTION

In recent years there has been a considerable study of the performance of a subject working in a closed loop control system. One of the difficulties of this type of investigation lies in how to present experimental findings in the most simple and direct way.

FIGURE 1
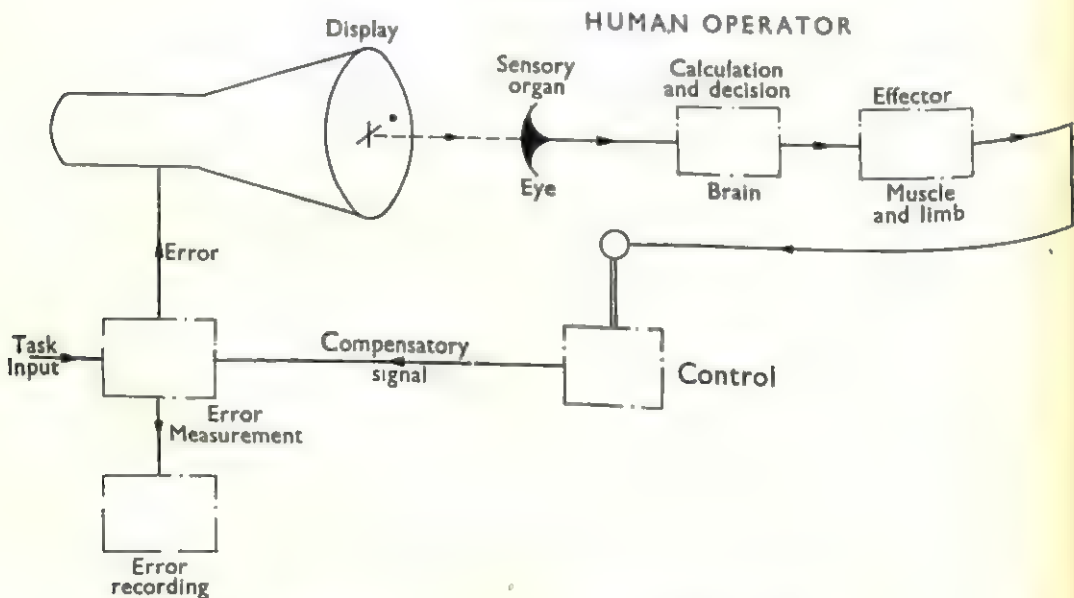
Schematic of typical closed loop control system involving the human operator

A simplified experimental situation for the study of a human operator in a closed loop is shown in Figure 1. A task $\theta_i(t)$ is usually generated by means of a cam or by some method of recording and playback and this is presented to the subject as a spot

position on a cathode-ray tube (C.R.T.). The subject also has a share in the control of the spot position by means of a control (e.g. a small joystick) which may compensate for deflections of the spot produced by the task signal. The response of the operator may be termed $\theta_o(t)$ and the difference between $\theta_i(t)$ and $\theta_o(t)$ gives the positioning error $\theta(t)$ which acts as the stimulus to the subject. This latter quantity may be recorded during the course of the experiment as, for example, a trace on paper and later analysed manually or automatically. Another way is to integrate the error and use the final sum as a measure of the type of response the operator has made. In this connection the usual criterion of performance is that of the root-mean-square error,

$$\left(\frac{1}{T} \int_0^T \theta^2 \, dt\right)^{\frac{1}{2}}$$

When this is computed information on the distribution of error at different frequencies is disregarded. In some cases this information may be valuable in assessing how best to use an operator. For example, a system involving the human operator might be improved by the introduction of a filter rejecting a certain band of frequencies which do not lie in the operating range of the system.

An ideal description of the performance of an operator would be one which was consistent with the method of describing linear servomechanisms so that the human part of a closed loop could be considered in a like manner to the rest of the loop and the overall performance assessed by well established theory. Tustin (1946) has dealt with such an approach, but unfortunately the general application of such a method seems to be precluded by the non-linear nature of the human operator. It is possible, however, to describe the performance of the operator by means of the spectral density of the errors he makes when carrying out a task and the task may be described in a similar way. By this method, of course, only the amplitude and frequency dependence of the signals is recorded and the phase relationship between say, input and response, is completely lost. However, the method has the advantage of showing not only the magnitude of error, but also its distribution in frequency.

In the present paper an attempt is made to describe briefly the equipment and technique necessary to produce such a spectral density curve or "power spectrum" of error. The typical spectrum of an operator attempting to maintain a fixed force with visual error information is presented as an example of the technique. It was thought desirable to seek an answer to the question of how a subject behaves when asked to perform a simple fundamental operation, namely, that of maintaining a fixed force with a visual presentation of error. This task was chosen because any external variation produced by an input $\theta_i(t)$ must necessarily produce complications peculiar to the type of input function introduced. Even a random input ("white"noise) increases the difficulty of interpretation of results on general lines and in any case this input requires modification at the higher frequency end and restriction in amplitude before presentation to a human operator.

II

APPARATUS

The apparatus has been fully described elsewhere (Sutton, 1955) and only the essentials will be dealt with here. It is important to point out that the equipment was designed originally as a general tracking simulator and not as a spectrum analyser. In consequence it may appear unnecessarily complicated in design if its more varied uses are not taken into account.

The apparatus consists essentially of a pressure joystick control, a number of high gain d.c. amplifiers, a C.R.T. display, an electronic squaring unit and a low inertia integrating motor with special compensating circuits. The recording and play-back section includes two motor-driven 35-mm. cameras together with C.R.T. and photo-multiplier equipment for producing the recording and also for playing it back by means of an electrical curve follower after development. Figure 2 shows the equipment arranged for recording a

FIGURE 2
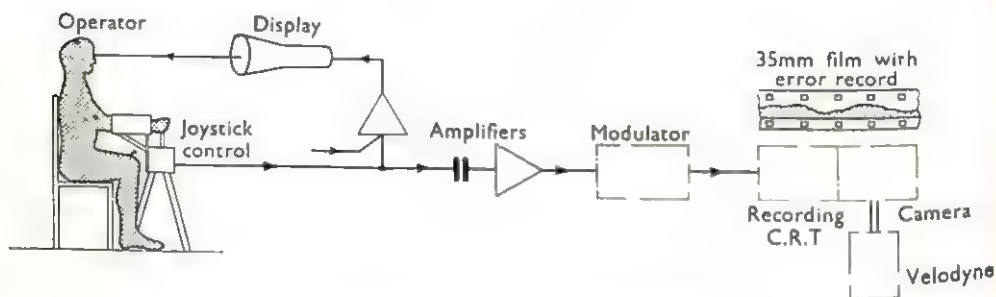
Simplified diagram of recording system

subject's response when asked to maintain a fixed force on the pressure joystick. Pressure is measured by means of an R.C.A. Mechano-Electronic Transducer Type 5734 whose voltage output is amplified and used to modulate the amplitude of a vertical trace on a small C.R.T. The natural frequency of the joystick when pulled over and freed is approximately 70 c/s as measured from the voltage output of the transducer. The C.R.T. trace is photographed on 35-mm. film, which is moved through the camera at a predetermined rate by a velodyne controlled motor system (Williams and Uttley, 1946).

After development the film is played back through the same system arranged as shown in Figure 3. The profile of the black image on the film is followed by an electronic

FIGURE 3
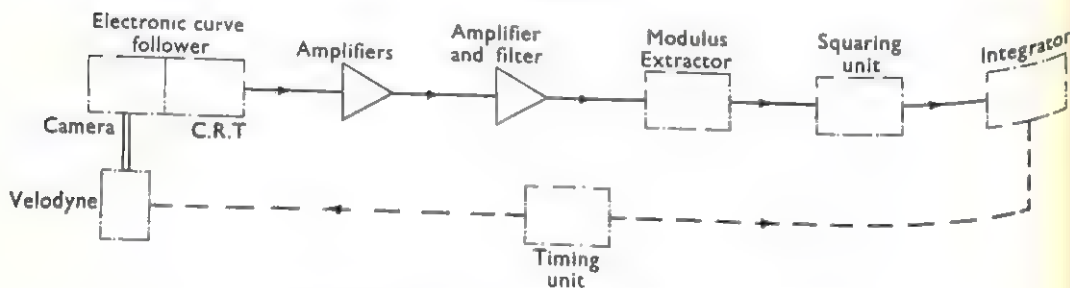
The play-back system used as a spectrum analyser

curve follower whose voltage output represents the original error variable. After amplification the signal passes through a filter of known bandwidth and central frequency and after further amplification is full-wave rectified. This signal is squared electronically and integrated by means of a low inertia integrating motor whose shaft revolutions are proportional to the time integral of the input signal. By playing the film through the equipment at various speeds with the same filter the integral of error squared at particular frequencies and for known bandwidths may be obtained. From this information the error "power" spectrum may be obtained by plotting the quantity

$$\frac{\dfrac{1}{T} \displaystyle\int_0^T \theta^2 \, dt}{\text{effective bandwith}}$$

against frequency.

The effective bandwidth is calculated from the following consideration. The actual bandwidth B of the filter is defined as the frequency difference between the two frequencies at which the filter output is 44·7 per cent. of the amplitude at resonance.

Let the speed at which the film was recorded be N_R. Then if the film is played back past the filter at this speed the signal extracted will extend over a frequency range B centred at f_o the resonant frequency of the filter. When the film is played back at any other speed N_P the effect is to multiply the frequency scale by a factor $\frac{N_P}{N_R}$ so that the filter will accept signals at a frequency originally at

$$f_o \cdot \frac{N_R}{N_P}$$

with an effective bandwidth given by

$$B \cdot \frac{N_R}{N_P.}$$

In practice it has been found better to fix the length of film under analysis accurately and set approximately the velodyne speed to give the required frequency. With this arrangement the analysis starts at a predetermined point on the film and stops automatically at the end of the measured length of film. The time T of the run thus determined is used to describe more exactly the frequency of the analysis and is also used in the above expression for the power spectrum.

In practice the range of frequencies which it has been necessary to cover has made it difficult to carry out the analysis with only one filter owing to the large range of speeds required. It has been possible to match filters sufficiently well to permit a change of filters at a convenient point.

III

MÉTHOD

The subject was seated in front of the C.R.T. display and the joystick unit was positioned on his right side with an arm-rest to support the forearm as far as the wrist joint, the wrist being quite free to move. The joystick was gripped over the top between the thumb, first and second fingers. In view of the fact that many muscles may be involved in movements of the wrist (some seventeen in all) it may be argued that this method of control is unnecessarily complex from the point of view of a possible extension of the work involving the study of, for example, electrical activity in the relevant muscles. While this is true it was also felt that the type of control chosen involved the use of practised movement (such as that used in writing) and any general useful conclusions from the experiment might more easily be applied to commonplace activities. However, in order to compare a simple movement depending on only a single muscle a similar method to that adopted by Merton (1954) was tried in which the same joystick was used, but pressure was applied by the thumb with the latter moving in the plane of the palm of the hand. The results showed a similar spectrum to that obtained by the joystick method.

The C.R.T. spot was moved to the right of the centre by a predetermined amount and the subject was asked to centralize the spot on a mark and maintain it there for 30 seconds. In order to do this the subject had to exert a force proportional to the initial spot displacement which in the present case represented 5 lbs. The gain of the system was such that a deflection of $\pm$ 2·5 mm. from the central mark was sufficient to produce a full-scale signal to the recording equipment. The force/deflection relationship with the gain used in this experiment was 0·4 oz./mm. so that the force change required for full modulation of the recording system was $\pm$ 1 oz. It will be noted from Figure 2 that the feedback path from the joystick control to the subject was direct, but the path to the recorder included an a.c. coupling. With this arrangement very slow drifts in the control system (i.e. small variations with time in the force/voltage output relationship of the joystick) would only affect slightly the total force being held up and would not overload the modulator.

Four male subjects were used in the series of experiments and sixteen experiments were performed on each of them over a period of two days. All the subjects were experienced trackers with this type of display and control system and all had had considerable initial practice in the present task. The arrangement was to record a complete group of one test on each subject in one period and then wait for at least 30 minutes before recording the

next group. In this way four recordings were made in the morning and four in the afternoon for two days and each subject therefore had a fairly representative set of tests throughout the day. The minimum interval between the tests seemed adequate to ensure that fatigue did not influence the results. Interrogation of the subjects showed that they were not aware of any feeling of tiredness in the wrist. Subsequent experiments confirmed that in 30 seconds the spectrum was unlikely to be affected by fatigue. In these experiments three of the same subjects were used and the same task set, but instead of a recording being made and analysed as a function of frequency the total error produced was squared

FIGURE 4
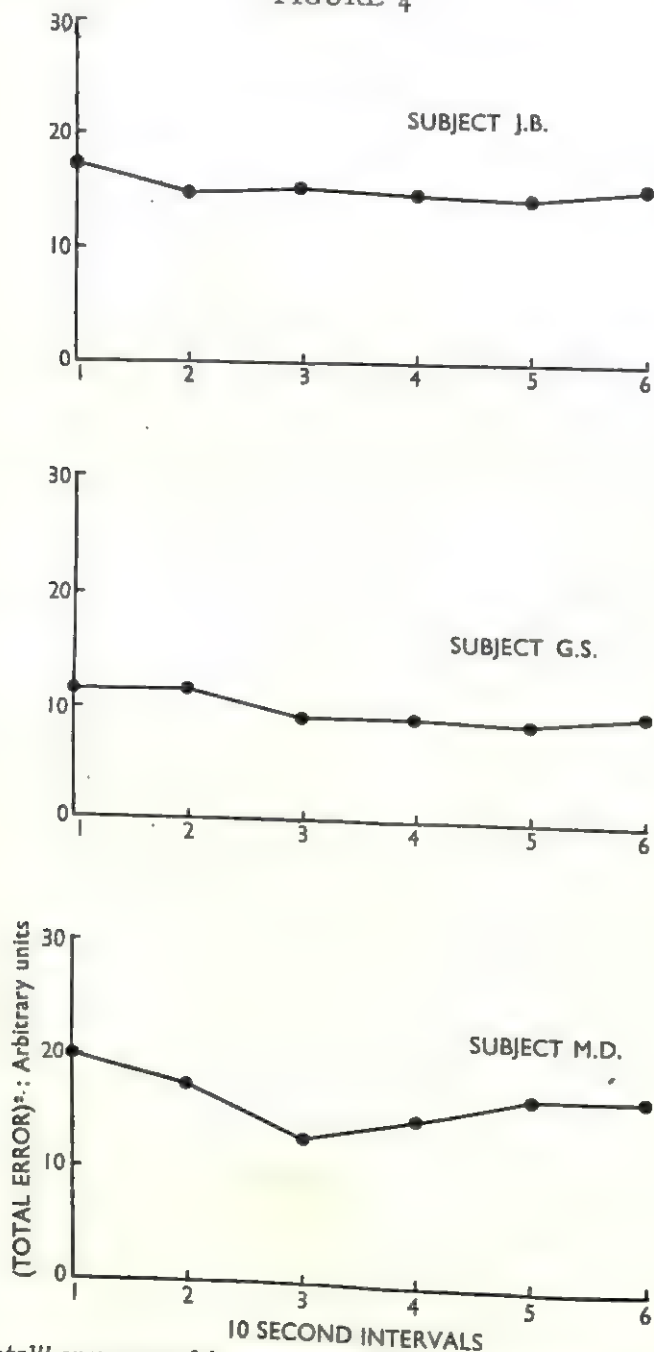
The "total" error scored in successive 10 second intervals. Each point is the mean of six observations. Force of 5 lbs.

and integrated directly, this integral being noted at 10-second intervals during the integration period. The results are shown in Figure 4 in which the total (i.e. unrestricted bandwidth) error "power" recorded in each of the 10-second intervals is plotted against the length of the experiment. The mean of the results for six experiments each on the three subjects shows no tendency for the error to increase with time over 1 minute and hence it is reasonable to suppose that the effect of fatigue on the main experiments was negligible.

IV

RESULTS

Each of the sixteen half-minute tests on the four subjects was analysed from 0·4 to 12·5 c/s in the manner already stated. Each test was plotted as a separate spectrum. The mean of the sixteen tests for each subject is shown in Figure 5.

The fact that the ordinates are given as error squared per unit bandwidth instead of root-mean-square error per unit bandwidth is not of great significance. The quantity plotted is in fact a measure of the variance of the subject performing the task plotted as a function of frequency. The results could well have been plotted so that the ordinates represented cms. of error on the C.R.T. or as force variations from 5 lbs. on the joystick. The main feature of interest is the general shape of the spectrum and the relative amplitudes of the errors at different frequencies. In order to bring out the latter point and to accommodate the large amplitude range required the results have been plotted on a logarithmic scale.

V

DISCUSSION

The spectra for the four subjects show certain common features. The first thing that is apparent is the concentration of error below about 3 c/s, this being not unexpected if the movements in this region are due to voluntary effort. Owing to the nature of the analysis it was not convenient or deemed very necessary to extend the measurements below 0·4 c/s. Consequently data at very low frequencies and information about bias is lacking in the results. However, it seems reasonable to suppose that the large amplitude errors at the lowest frequency measured must be very much reduced at 0 c/s. This has been confirmed by a small number of "live" experiments (i.e. without recording and with no filter) in which the same subjects have shown under the same conditions that their mean error is small compared with their total r.m.s. error.

The other noticeable feature of the spectra (and particularly those for subjects M.D. and J.B.) is the small peak in error over the region 7 to 9 c/s. Hand tremor at this frequency has been well observed by other workers (e.g. Travis and Hunter, 1931; Sollenberger, 1937; Redfearn and Halliday, unpublished work). Its origin in normal subjects is at present uncertain, but if muscular effort is governed by servo principles the fact cannot be ignored that tremor may be due to sustained oscillation in the system or to selective amplification of certain frequencies of efferent activity. If this is the case then interest in the peak lies more in its bearing on muscle servo performance than in its contribution to the total error which in the present case is shown to be small.

In order to obtain some idea of the relative importance of errors incurred over different frequency bands the results obtained for one subject (J.B.) were plotted as r.m.s. error/unit bandwidth against frequency and an approximate measure was made of the area of the graph lying between certain frequency limits, the results being shown

FIGURE 5
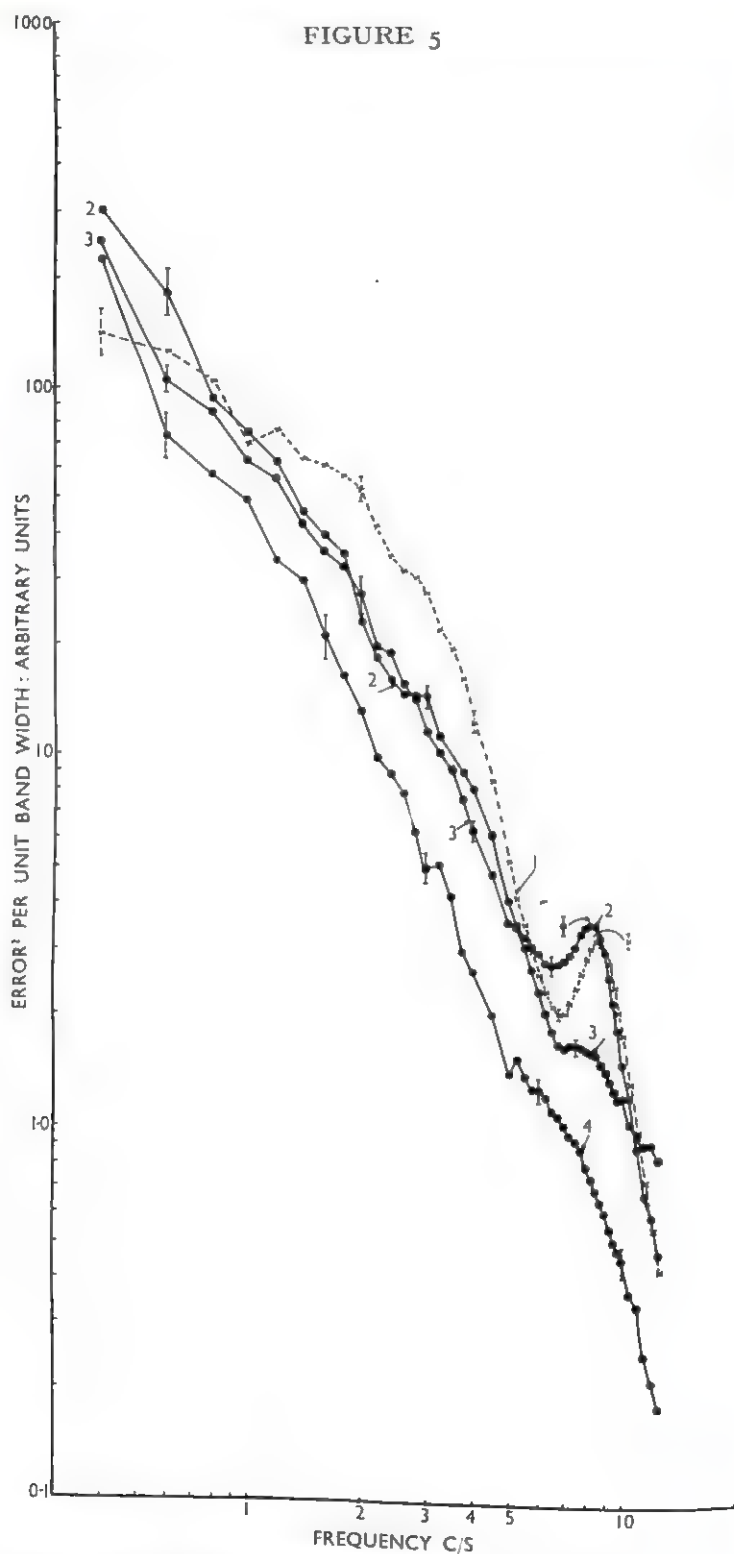
Error power spectra for four subjects 1. M.D., 2. J.B., 3. G.S., 4. K.S.
Each point is the mean of 16 observations. The bars represent the
standard deviation of the mean. Force of 5 lbs.

in Table 1.   Table 2 shows the percentage of total area lying between certain frequency limits.

TABLE I

Frequency Range	Area
0·4 to 0·8 c./s. · · · ·	10·5
0·8 to 2·2 c./s. · · · ·	19·0
2·2 to 3·75 c./s. · · · ·	12·0
3·75 to 5·5 c./s. · · · ·	8.0
5·5 to 12·5 c./s. · · · ·	17·0
9 c./s. peak (above "base") · ·	2·5
Total · ·	69·0

TABLE II

Frequency Range	Percentage of total in this Range
0·4 to 2·2 c./s. · ·	43
0·4 to 3·75 c./s. · ·	60
0·4 to 5·5 c./s. · ·	72
9 c./s. peak · ·	3·5

Even ignoring the activity which lies below 0·4 c/s. leaves nearly half the total error between 0·4 and 2·2 c/s. An estimate has been made of the increase due to the 9 c/s. peak by drawing a "base" line connecting the error levels before and after the increased activity.

The contribution of this activity to the total error is small, but if a system with a resonance near the frequency of this peak were to be controlled considerable amplification of the effect might take place with consequences which could not be ignored. It is also worth noting that the present system measures displacement of the fingers (or force exerted by them). In a system including a higher derivative control (e.g. an accelerometer mounted on the hand) the higher error frequencies in the system would necessarily be accorded a high gain and the tremor peak would then produce an undesirable contribution to the total error.

Although the subjects were thought to be fully trained in this particular task it was thought interesting to see what day-to-day variations took place. Accordingly, an experiment was arranged which involved each subject in four tests per day for five days evenly disposed throughout the morning and afternoon of each day. In this case the measure of performance was total error squared in attempting to maintain a force of 5 lbs. with C.R.T. monitoring. The results are shown in Figure 6. In the case of two subjects (M.D. and J.B.) there seems no sign of learning taking place. In the case of the other two subjects, however, there appears to be a general trend towards a lower score per test even after the fifth day. In view, however, of the large variations between one test and another it was felt that there was little to be gained by increasing the training period of these two subjects before recording the tests from which the spectra have been derived.

FIGURE 6
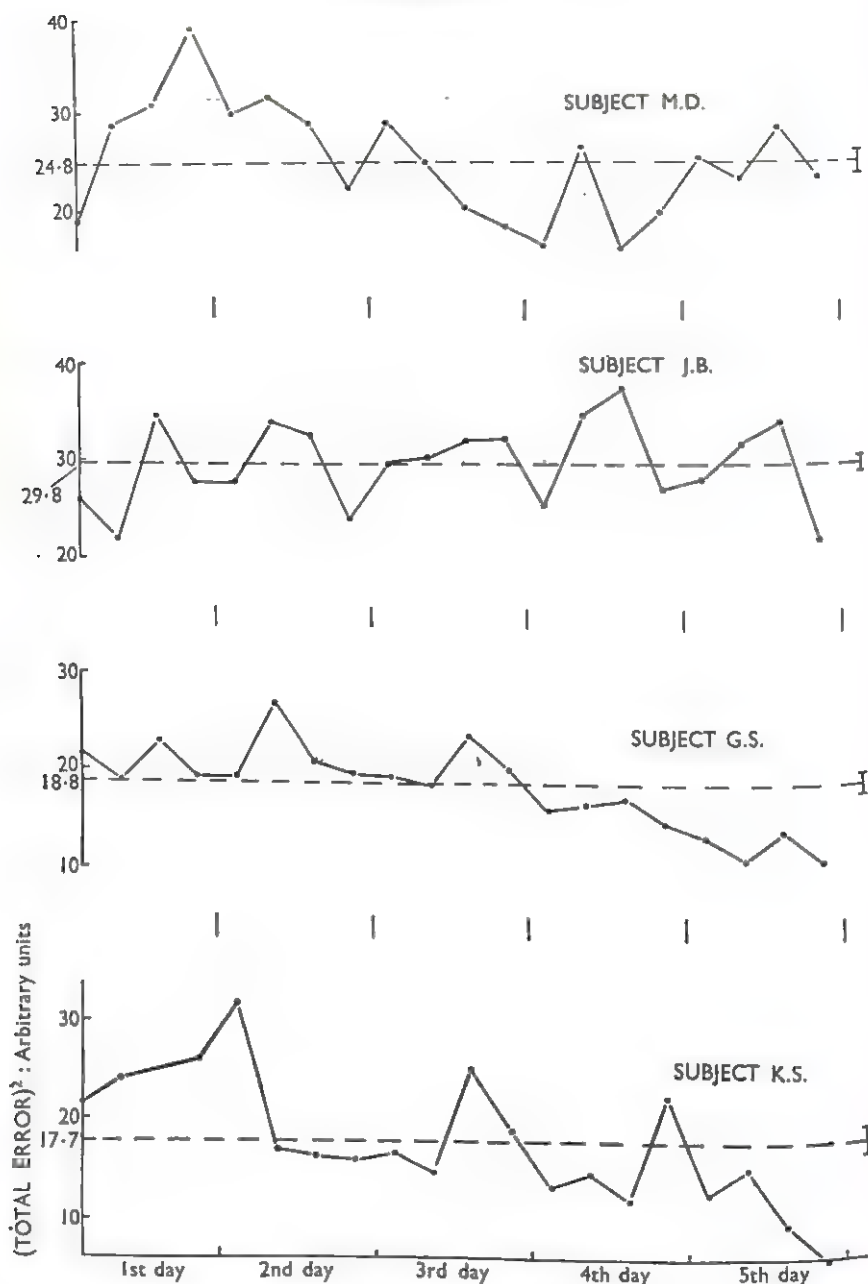
Day to day variation of "total" error scores. Force of 5 lbs. maintained for 1 minute in each case. The mean score and standard deviation of the mean are indicated for each subject

It is interesting to observe that there is a marked tendency for the standard deviation bars in Figure 5 to increase toward the lower frequency end of the spectrum. Since the ordinates are plotted on a logarithmic scale this implies that fluctuation in performance at any frequency is not a fixed proportion of the error amplitude. It is tempting to suggest that the increase in reproducibility of the spectrum towards the

higher frequency end is due to the transition from a largely voluntary region of activity to one of involuntary activity. It certainly appears from the results for the subjects with a large amount of tremor that this region is the most reproducible one in the whole range investigated.

My thanks are due to my colleagues, Mr. K. Sykes, Mr. J. Biscoe and Mr. M. Dew, for their valuable help during the course of this work.

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THE EFFECT UPON MONAURAL SENSITIVITY OF CONTINUOUS STIMULATION OF THE OPPOSITE EAR

BY

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The threshold to a 1,000 c./sec. tone presented to the left ear was measured whilst the right ear was under continuous stimulation by a 400 c./sec. tone. Observations were made, on different groups of subjects, under three stimulus conditions and two conditions of attention. Thresholds were found to increase with increasing intensities of the continuous tone. Attention to that part of the field associated with the continuous stimulus produced no significant change, whether the continuous stimulus was present or not. There were no significant after-effects during ten minutes following the end of the continuous stimulus, though the results suggested a slow decrease in threshold.

Several explanations of the phenomenon are considered. Cross-hearing and the reflex contraction of the middle-ear muscles can be virtually excluded. Central inhibition or the central control of sensory end-organs can account for the results. A statistical hypothesis is also tenable.

PROBLEM

Stimulus interaction experiments have been widely used in attempting to understand the functioning of the nervous system. The basic requirement of such an experiment is a test stimulus which, under constant conditions, produces a consistent and observable response. Another stimulus is then applied to the organism and any change in the response to the test stimulus is observed.

The method has met with its most striking success in the work of Sherrington (1947) on the spinal cord. With the aid of Sherrington's theories it is possible to predict the response of a spinal animal to different combinations of stimuli. Pavlov (1927) attempted to do the same for brain function, using intact dogs and the conditioned reflex method, but did not develop his theory to the point at which precise predictions can be made.

He does not appear to have specified unambiguously the conditions under which an inhibitory or a facilitatory effect would be expected, although both were observed in his experiments.

Similar effects have been reported in human subjects, using thresholds rather than conditioned reflexes. Investigations where the two stimuli were in different modalities have been reviewed recently by Symons (1954), some showing inhibition and others facilitation. Investigations of interaction within one sensory modality show similar discrepancies (Ryan, 1940). Here again it seems impossible as yet to specify the conditions which produce these opposite effects.

Two of the factors which seemed, from a review of the literature, likely to influence interaction phenomena were stimulus intensity and attention. The object of the present investigation was to study the effects of these two variables upon stimulus interaction within the auditory modality.

The hypotheses to be tested were (1) that sensitivity to a stimulus is less when another sector of the stimulus field is being attended to and (2) that sensitivity to a stimulus varies inversely with stimulus intensity in another sector of the field. Clearly *attention* and *stimulus intensity* are likely to be related. For example, in a varied stimulus field it seems probable that the more intense stimuli will get a greater share

of attention.   Any attempt to control attention may thus have different effects depending upon the stimulus intensities concerned.   We might therefore predict (3) that there is an interaction between attention and stimulus intensity.

METHOD

The test stimulus was a 1,000 c./sec. tone presented to the left ear.   Threshold intensities of this stimulus were measured nine times whilst a tone of 400 c./sec. was presented to the right ear as follows:

(1)   No continuous tone; occasional "pips" at 0 db. to facilitate the control of attention.

(2)   Continuous tone; 10 db. above threshold.

(3)   Continuous tone; 30 db. above threshold.

The conditions of attention were determined by the instructions given to the subjects which were:

either directed attention; to count the number of "pips" or changes in pitch.   The latter were occasional changes of about 10 c./sec. in the continuous tone,

or undirected attention; to ignore the 400 c./sec. stimulus.

Each subject was allotted at random to one of the six possible combinations of stimulus and attention conditions.   To reduce the variance due to individual differences in threshold, all subjects were first given a control series of nine threshold measurements under identical conditions without stimulation of the right ear.   In order to observe possible after-effects, a further series of nine determinations was made following the end of the stimulus in the right ear.

During a preliminary practice period, the threshold of the right ear to the 400 c./sec. tone was measured and nine determinations of the test stimulus threshold were made.   At least ten minutes were allowed to pass after practice, before the start of the experimental observations (*see* Table I).

TABLE I

OUTLINE OF EXPERIMENTAL SESSION

Practice series
Rest period (at least 10 minutes)
Series   I: Control
Series   II: One of the six combinations of
                stimulus intensity and attention
Series III: Control

Subjects

Members of the female nursing and laboratory staff of Whitchurch Hospital were asked to take part in the experiment.   Results from 48 subjects, whose ages ranged from 19–58 years, were included in the final analysis.   No attempt was made to screen the subjects for otological abnormalities, but serious hearing defects were detected during the course of the experiment and subjects suffering from these were not included.   Ten subjects were excluded because mistakes were made in the experimental procedure.

Threshold measurements

The test tone was produced at 1,000 c./sec. by means of a Dawe Wide Range Oscillator. The output from the oscillator was fed through a 5,000 ohm resistance to a Marconi attenuator with a 600 ohm input and output impedance.   The input to the attenuator was monitored by a valve voltmeter and the output was fed to a moving-coil earphone (Brown's type K), through a 580 ohm resistance to give the correct termination impedance for the attenuator.

Thresholds were determined by the method of limits using intensities increasing in steps of 1 db.   Each intensity was presented for 5 secs.   The interval between one response and the beginning of the next determination was 15, 30 or 45 seconds, randomized throughout the session.   The starting point for each determination was always 6 db. below the previous threshold.   The same order of time intervals was used for all subjects. The subject was given no preliminary signal before each determination and was told, at the outset, not to make an effort to listen for the sound but merely to report whenever the subject was given no preliminary signal before each determination and was told, at

she did happen to hear it. It was thought that, with these instructions subjects would be less confused when told, later, to concentrate on the other stimulus, whilst continuing to report the presence of the test stimulus.

Instructions

After familiarizing the subject with the tone and finding the approximate threshold, the following instructions were given:—

"Well, from now onwards I don't want you to listen for this sound; don't *try to* hear it, just think about anything you like. Day-dream if you like. Don't concentrate on anything in particular. But whenever you do happen to hear this sound, that's the one in your left ear that you just heard, say yes. Do you get the idea? Don't try to hear it, but whenever you do happen to hear it, let me know. O.K.? We'll try it then. Don't say unless you're *sure* you can hear it. Don't expect it to come very often, it will only come occasionally."

It was decided at the beginning to exclude any subject who made more than three false responses (i.e. reporting the presence of the tone when it was not, in fact, present) during any of the series I, II or III. In fact, no subject made more than two. Three made two false responses and eight made one.

Stimulus to right ear

This was produced by means of a tape-recorder, the output being fed to the opposite earphone of the same headset, through an attenuator which could be changed in steps of approximately 10 db.

For the continuous tone, a sine wave of about 400 c./sec. was recorded on the tape, lasting for 12 minutes. During the first 9 minutes of this there were 30 frequency changes (up and down alternately) of about 10 c./sec. These were distributed at random throughout the 9 minutes. The threshold for this stimulus (right ear) was determined first in the experimental session. With maximum gain on the tape recorder, the attenuator was set to the maximum position at which the subject could just hear the tone. The threshold was then determined by the method of limits, using three ascending and three descending series of intensities. Intensities were varied by the gain control of the tape recorder. With the gain at the average of these six readings, the attenuation was now reduced by either 10 db. or 30 db. as required by the prior allocation of the subject to one of the groups. She was asked to say whenever the pitch of the note changed and this was continued until the experimenter was satisfied that the frequency changes were being detected correctly.

For the first stimulus condition (absence of continuous tone), it was necessary to ensure during series II that the subjects in the "directed attention" group were attending to part of the stimulus field other than that occupied by the test stimulus. Very short bursts of a 400 c./sec. tone were recorded on the tape, the intervals between them being the same as those between the frequency changes of the continuous tone. The subject was asked to say whenever a "pip" was heard, and the threshold was determined by increasing the intensity until three out of three were reported. This intensity was used for series II.

RESULTS

Stimulus effects

The problem was to see how sensitivity to the test stimulus changed with different intensities of the other stimulus, under two conditions of attention. The data analysed were the series II thresholds (right ear stimulated), expressed as deviations from the series I thresholds (right ear unstimulated). Thresholds were measured in db. below a reference sound pressure level of approximately 1 dyne/cm.^{2}

The dependent variable was the sum of series I thresholds minus the sum of series II thresholds and the two independent variables were stimulus intensity (three categories) and attention (two categories). Series I thresholds did not vary significantly between groups. The results of an analysis of variance of the differences are shown in Table II. There was reason to doubt some of the assumptions made in orthodox

analysis of variance. There were insufficient subjects to enable any conclusion to be drawn concerning the distribution function of threshold differences, but the distribution of mean thresholds (series I) was slightly skewed.   There were larger differences than would be expected by chance between threshold difference variances of the six groups, the 30 db. "directed attention" group having a larger variance than the remainder.   The H-test (Kruskal and Wallis, 1952) gave substantially the same results, however, for both stimulus intensity and attention, though it is not possible to test the significance of the interaction variance using this method. This test does not involve an assumption of normality and is little affected by differences in variance between groups.

TABLE II

ANALYSIS OF VARIANCE: SERIES I MINUS SERIES II

Source	df	Variance	F	P
Attention	1	1,938	1·21	> 0·05
Stimulus intensity ..	2	8,871	5·52	< 0.01
Interaction	2	753	—	—
Within groups ..	42	1,607	—	—

Average thresholds throughout the three series are shown in Figure 1.   A decreased sensitivity during series II is shown for all four groups stimulated by the continuous tone.   The other groups, however, maintained a relatively constant level.

It is concluded that hypothesis 2 is verified, but that there is no evidence for 1 or 3.

After-effects

Series III thresholds were also expressed as deviations from series I thresholds. The difference between the sums of thresholds in series I and III did not show a significant variation due to either attention, stimulus intensity, or the interaction between the two (see Table III).   The H-test confirmed this, for attention and stimulus intensity effects.   The graphs in Figure 1 show, however, that after-effects might be obscured by considering only the sum of all series III thresholds.   Following the end of the continuous tone there appears a tendency for sensitivity to rise from a point below the initial mean level to one considerably above it at the end of the series.   Neither of the unstimulated groups show such a consistent trend.

TABLE III

ANALYSIS OF VARIANCE: SERIES III MINUS SERIES I

Source	df	Variance	F	P
Attention	1	0	—	—
Stimulus intensity ..	2	2,214	2·12	> 0·05
Interaction	2	614	—	—
Within groups ..	42	1,044	—	—

FIGURE 1
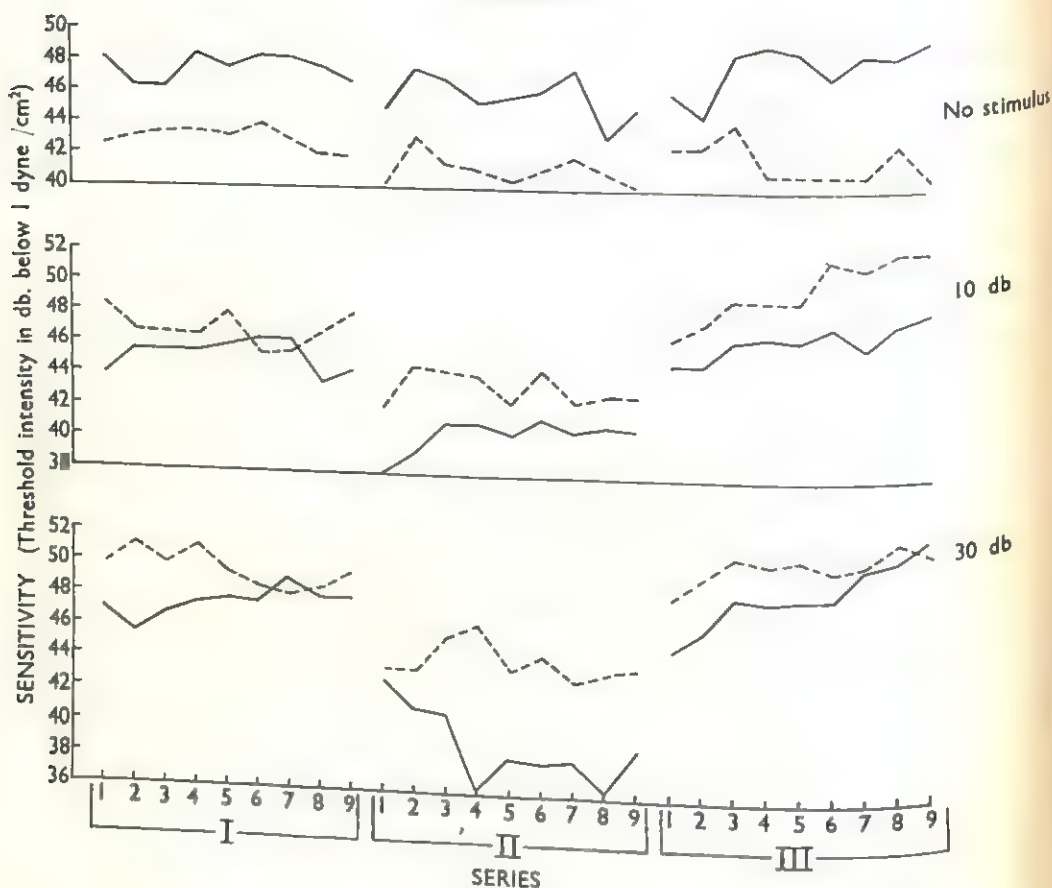
Average thresholds for the six groups. Intensity of 400 c./sec. tone in opposite ear during series II is shown on the right of the graphs.
Solid line—directed attention during series II.
Broken line—undirected attention during series II.

A further analysis was done to see whether effects, either of stimulus intensity or attention, could be detected on either the first (III_1) or the last (III_9) post-stimulation thresholds. These were expressed, as before, as deviations from the average of series I. No statistically significant effects were found, using either analysis of variance or the H-test.

Regression coefficients for thresholds on position of observation in series III were calculated for each subject. The signs of most of these coefficients were positive (showing an increase in sensitivity during series III) for the four groups to whom the continuous tone had been applied in series II. There were two exceptions in each of two of the groups and one exception in each of the other two. In the unstimulated groups there were slightly more negative than positive signs. An analysis of variance showed no significant inter-group differences, but using the Kruskal and Wallis method, H was 5·70 (almost significant at the 5 per cent. level) for stimulus intensity.

It is not possible to reach any definite conclusion on after-effects, but the evidence of an increase in sensitivity during the first ten minutes following the continuous tone is strong enough to justify further investigation.

DISCUSSION

The results show evidence of an inhibitory effect of one auditory stimulus on another, when the two stimuli are presented to opposite ears. Possible explanations of this phenomenon will now be considered.

Peripheral explanation

The masking of one tone by another has been widely observed when both tones have been presented to the same ear. It seems to have been accepted, however, following the work of Wegel and Lane (1924) that masking does not occur when the tones are applied to opposite ears. Licklider (1951) states in this connection that "what little masking is observed can be attributed largely to leakage of the masking vibration around the head." Wegel and Lane compared the masking curves (stimulus intensity against masking effect) for binaural stimulation (tones applied to opposite ears) and monaural stimulation (tones applied to same ear) and found that the curves for the former were displaced about 40 db. from those for the latter. This would be predicted on the assumption that the binaural effect was due to cross-hearing as suggested by Licklider. However, two instances were reported, using notes of widely different frequencies, where this displacement did not occur. Also, these results were obtained from one observer and, as was found during the present work, there are wide individual differences in interaction effects. Although, therefore, Wegel and Lane failed to observe definite evidence of binaural masking, their results are not inconsistent with the existence of such an effect under certain circumstances.

If we are to infer from our results anything about the functioning of the central nervous system, it is necessary to exclude the possibility that the ear to which the test tone was applied was also being affected by the masking tone. Lane (1925) used the method of "objective binaural beats" to determine the amount of sound reaching one ear during stimulation of the other by headphone. The attenuation between the two ears varied from 44 to 75 db. for a number of observers and frequencies from 200 to 4,000 c./sec. In a person totally deaf on one side he found a difference of 58 db. in the thresholds when a tone was applied to the two ears. From this it was concluded that a tone 58 db. above threshold applied to the deaf ear stimulated the normal ear at threshold intensity, thereby giving a rough confirmation of the figures obtained by the other method. Békésy (1948), using the method of beats, reported an attenuation of 40–50 db. from one side of the head to the other.

The same method was used to measure the amount of cross-hearing obtained during the present experiment. It was found that the point of maximum beats was difficult to determine accurately but, erring always on the safe side (i.e. towards the minimum attenuation from one side to the other), figures ranging from 33 to 55 db. were obtained from three subjects for 400 and 1,000 c./sec. It is probable that the true attenuations were greater than 40 db. in all cases.

During the experiment, one subject was found who had a perforated tympanic membrane on the right side. A 1,000 c./sec. tone applied to the defective ear was reported as coming from the opposite side, the threshold intensity being 50–55 db. greater than that for the same tone applied to the normal ear. A 400 c./sec. tone was not heard at all when applied to the right ear even when its intensity was 52 db. greater than the left ear threshold (this was the maximum intensity obtainable from the apparatus). Another subject was found to be deaf in the right ear for 1,000 c./sec. tones. The threshold intensity of a tone of this frequency was 50 db. greater when applied to the right ear than when applied to the left ear. The difference for a 400 c./sec. tone was only 10 db., but it seemed that in this case the sound was heard in the right ear.

To summarize the evidence on cross-hearing, it seems likely that a sound reaching one ear from one of these headphones applied to the opposite ear is attenuated by more than 40 db. As the greatest stimulus intensity was 30 db., the sound reaching the opposite ear direct must have been subliminal and insufficient to produce a masking effect. It follows from this that the masking effect observed can hardly have been due to peripheral events entirely.

Peripheral-central explanations

Reflex contractions of the stapedius and tensor tympani muscles, elicited by sounds and mediated *via* the superior olive, have been shown to be bilateral, a sound to either ear causing contractions on both sides. Experimental studies of the tympanic muscles have been reviewed in some detail by Wever and Lawrence (1954). They conclude that the function of this reflex is to control the efficiency of energy transmission in the ear, usually reducing it and thereby protecting cochlear structures from excessive damaging stimulation.

The reflex can be observed indirectly as a change in the acoustic impedance of the ear (Metz, 1951). Jepson (1951), using this method in 61 normal adults, found that the threshold intensity for the reflex was about 70 80 db. greater than the hearing threshold. These figures have been confirmed in cases where perforations in the tympanic membrane permitted direct observation of the muscles. As none of the tones used in our investigations was more than 30 db. above the threshold, reflex contractions of the tympanic muscles can be excluded as an explanation of the inhibitory effect observed.

Another form of central control of sense organs has recently come under investigation. This is the direct influence of the brain, either inhibitory or facilitatory, on sensory end-organs (Granit, 1954, 1955). Such influences have been demonstrated electrophysiologically for muscle spindles and retina. It is impossible to say, at present, whether the receptors in the organ of Corti or the cell bodies of the auditory nerve fibres can be influenced in this way, but a possible structural basis for the latter exists in the centrifugal fibres which have been described terminating in the spiral ganglion (Davis, 1951). The hypothesis that the inhibitory effect is caused by the central control of peripheral structures must, therefore, be regarded as tenable.

Statistical explanation.

The sensory nervous system is in a state of constant activity, partly induced by external and internal stimulation, but also due to spontaneous discharges. This activity can be regarded as a background of "noise" against which the effects of a new stimulus must be discriminated. The organism is thus faced with a statistical problem. If, in one sensory system at a given time, a group of n neurons is more active than the remaining N neurons in the system, is this a result of random variations within the system or are the n neurons being activated by an additional external stimulus? In other words, do the two sets of activity rates come from the same population or not? Assuming a mean increase of neural pulse rates ΔR corresponding to a threshold stimulus intensity ΔI,

then
$$\frac{(R + \Delta R) - R}{\sqrt{\left(\frac{1}{n} + \frac{1}{N}\right)V}} = C$$

where C = a constant determined by the number of permissible observational errors.

R = mean neural pulse rate over the population of unstimulated neurons.

V — variance of neural pulse rate over the same population.

Thus, the judgement of "present" or "absent" depends not upon the stimulus intensity alone, but upon the ratio C. The addition of a second stimulus activates some of the N neurons which are, therefore, no longer available in the null population. Alternatively if N remains the same, the stimulated neurons produce an increased V. In either case a higher ΔR, and therefore a higher ΔI, is then required before the threshold level of C is reached. This formulation of sensory thresholds was recently suggested by Gregory and Cane (1955), who showed that, in the case of vision, the laws relating stimulus area and threshold (Ricco's law and Piper's law) could be accounted for in these terms.

Whether the stimulation of one ear by a relatively pure tone is sufficient to cause appreciable reduction of N or increase of V is uncertain. With our present knowledge of the neurophysiology of hearing, however, this must be regarded as a possible explanation of the results.

Central explanation

Finally, the effect may be due to interaction occurring within the brain, a form of central inhibition. Pavlov postulated the existence of such interactions in the cortex, to explain the inhibitory effect of one stimulus on the conditioned reflex elicited by another.

This "external inhibition" has been explained by Broadbent (1953) by a "competing stimulus" theory. According to this theory, the organism is not able to respond to all incoming stimuli at any one time, but must make a selection. The probability of a response to one stimulus is reduced by the simultaneous occurrence of another stimulus of "high priority." The priority of the stimulus is related to factors such as stimulus intensity, biological importance and novelty. In the present experiment it was expected that the condition of directed attention would favour the selection of the right ear stimulus and produce a greater degree of masking in the left ear than undirected attention. There is no evidence from these results that attention to one part of the stimulus field lowers the sensitivity to stimuli in another part when stimulus conditions are otherwise constant. This does not necessarily disprove a "competing stimulus" type of explanation, but it does suggest that if such an explanation is true, the nature of the stimlulus itself is more important in determining its "priority" than the instructions given to the subjects in this experiment.

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BOOK REVIEWS

An Introduction to Cybernetics. By W. Ross Ashby. London: Chapman & Hall, 1956. Pp. 295 + ix. 36s.

Referring to my comments in this *Journal* on Dr. Ashby's previous book, *Design for a Brain* (such is the type-casting of reviewers), I find that certain observations are still applicable. Thus it is equally true of the present book that "it should be judged rather as an attempt to find a fitting and adequate language (for the analysis of complex systems) than as a specific message in that language"; and there is still "a good deal of padding." On the other hand, the author's claim that the two books are complementary rather than overlapping is fair. This feat—for it *is* a feat—has been performed by adventuring further into the almost unlimited field of mathematical logic, leaving brain-designing as a comparatively special case. The result is no more a psychological or physiological work, in the ordinary sense, than a treatise on "medical statistics" is a medical book; both are mathematical works got up to attract a particular class of reader—or purchaser. Thinking of one's probable readers is highly recommended by those who pretend to teach us how to write; but it is dangerous advice. The proper advice—if advice is called for at all—is: "Say what you have to say in the manner you would like if *you* were the reader; or, if that defeats your imagination, imitate. But never, unless you really know what you are doing, try to overcome prejudices by persuasive reasoning; not only will you probably fail, but you will try the patience of those who do not have those prejudices and who are the only readers you really value." Ashby begins his Preface with the remark that many biological scientists would like to apply cybernetics, but have been deterred by the impression that it needs a long study of electronics. I can confirm that certain eminent people, including neurophysiologists, drag in electronics in very much this way; it is similar to saying to a keen fly-fisherman: "I would like to take up your sport, but I could never get the hang of worms and floats and all that." In short, such ignorance, expressed so, belies the initial claim to be interested, and the expert is justified in wasting no time on such a man. Likewise, no-one with the type of mind needed to get anything useful out of cybernetics could be so grossly stupid as to suppose that it had any necessary dependence upon electronics. Plenty of people, highly astute in other directions, might and do quite pardonably labour under this misconception; but as they will go their own ways in any case, why try to convert them?

This is but one example of many throughout the book; they are blemishes because they confuse, irritate, and occupy a great deal of unnecessary space—and therefore presumably add to the price, though that depends also on the sales. The straining after a mathematical simplicity that cannot be, and is not, achieved without sacrificing some clarity and much practical utility is in the same category. The author carefully avoids the calculus, although it is familiar, more or less, to many biologists (and is, in any case, an easy step from schoolboy mathematics); yet most of the book is devoted to symbolic transformations which, though they look innocent, are far more abstract, and consequently more difficult to apply to most real problems of importance.

The late A. A. Milne once accounted for the tendency of dramatic critics to tear a serious play to shreds, while cheerfully eulogizing the latest musical comedy, on the ground that the one was worth dismembering, while the other was beneath their professional notice. My criticisms of this book are to be taken in the same spirit. It contains much of value to those who are interested in these matters; I only wish the author had let himself go a bit more.

W. E. Hick.

Receptors and Sensory Perception. By Ragnar Granit. Yale University Press: London. Cumberlege, 1955. Pp. 369 + xii. 40s. net.

A new book by Professor Granit is bound to be an exciting event for psychologists. Not only do his numerous experimental contributions cut right into problems of nervous function which are basic for any real understanding of behaviour at the level studied by psychologists, but Granit's own early interest and experience in our field gives his exposition a particular orientation. It should be said at once, however, that this does not make

his Silliman Lectures any easier reading. The book is in fact an extremely difficult one, and one may suspect that not only psychologists will find it so. It would be a pity nevertheless if anybody were deterred from giving it the close attention it deserves.

Readers of *Sensory Mechanisms of the Retina* will recall the extent to which Granit, in the midst of detailed and specialized investigation into the minuter aspects of sense-organ function, strove to discern and establish a general picture and some basic principles. To a greater extent this is true of the present work, and the rapid advances of the intervening ten years offer much material—an abundance, indeed, which is sometimes embarrassing. The book centres largely around the question of the control of the state and activity of sense organs by impulses reaching them from the central nervous system, a topic whose importance generally may well become outstanding in the next phase of advance. For the present it can be discussed chiefly in terms of the γ-system associated with muscle spindles. But apart from control mechanisms such as the pupillary and accommodatory reflexes, and the reflexes of the middle-ear muscles, histological evidence of centrifugal fibres in both optic and auditory nerves gives grounds for suspicion that the visual and auditory receptors may themselves be subject to direct control of central origin—may possess "tonus" so to speak, whose variations afford them greater efficiency in their task of transmitting information in a variety of conditions. In the meantime, experimental study by Hunt and Kuffler, by Granit and his colleagues, and by others of the details of the γ-system, by which the state and activity of the muscle-spindles are adjusted by impulses of central origin, have led to solid achievement in as much as a number of puzzling features in the phenomena of posture and movement have been elucidated.

The existence of inherent control loops in sensory systems does not, however, exhaust the respects in which the recent research expounded by Granit at times clarifies, and at others makes more obscure, our ideas about the working of these systems. Apart from the classical problem, whose early simplicity recent work has largely dissolved, of the transformation of elementary stimulus events into corresponding elementary neural processes, a number of other questions now hold the field. There are, for instance, the problems of what Granit calls "organization" at the sense organ level. And there is the problem of spontaneous activity—to mention but two.

By the word "organization," the author means something at once broader and more specialized than the conception ordinarily attached to it by the psychologist. He is referring to all those processes whereby the elements, out of which from a purely physical point of view the stimulus may be regarded as made up, are re-sorted in the transducer mechanisms and appear in the peripheral nerve signal in fresh arrangements presumably more appropriate to their functional destinations. So, for instance, in the retina, a light stimulus of given intensity and duration is transformed into a pattern of neural activity corresponding to a combination of "on," "off" and "on-off" effects, transmitted in separate pathways. Again the temperature and pressure of an object applied to the skin will, according to the particular values of the variables, arouse differing combinations of activity in a number of different specific mechanisms and the whole information transmitted about the object is contained in several separate channels. Another form of "organization" in the retina would be exemplified by the interaction, *via* the ganglion level, of neighbouring elements. It may be said perhaps that Granit's use of the term "organization" at the peripheral level refers to the sum-total of all those respects in which the receptor processes depart from a single "point-point" "through-route" principle. If this is to understand Granit aright, it seems a pity that some other term could not have been found, though admittedly it is difficult to think of an adequate one. The forms of analysis and subsequent re-synthesis suffered by the stimulus pattern are so diverse that to characterize them generally, except by merely denying or limiting a simple constellation hypothesis, seems at present all but impossible.

The role of spontaneous activity in the sense-organs is considered at some length, and the provisional conclusion reached that it may serve to maintain a background of activation of higher central mechanisms. These latter in turn, it is suggested, can exercise via the appropriate centrifugal pathways an influence on the sense-organs and regulate their condition of activity. "The organism itself can adjust the level of permanent firing to its needs."

It will be seen that the author combines bold speculation over a wide field with close argument based on detailed and coherent experiment where this stands to hand. The latter is unfortunately at present available only in severely restricted fields, and its possible relevance to broader problems is suggested by somewhat tenuous links, such as the (histological) evidence of centrifugal pathways in other systems, notably the auditory.

and the visual. But ". . . ten years hence we shall know a great deal more about these problems" and, ". . . constructive speculation may not at the moment be out of place." It is the juxtaposition of constructive speculation against hard fact that makes this book at once somewhat forbidding and deeply attractive.

It should be added that included among the lectures are two, somewhat segregate, which contain accounts of the present positions regarding the dominator-moderator problem, and the electro-retinogram. The series concludes with a somewhat general and discursive attack upon the mechanisms of discrimination. As a whole the book is clearly a most significant contribution to forms of thinking about the nervous system in relation to behaviour. In it we seem to catch glimpses of the possibility of a return to unitary principles in the interpretation of sensory function, and such principles may turn out to be deeply suggestive in the study of other fields of behaviour.

R. C. OLDFIELD.

PROCEEDINGS OF THE EXPERIMENTAL PSYCHOLOGY GROUP, 1956

27th–28th March, 1956. Extended Meeting at Oxford. *1st Session:* "Further analysis of the effects of frontal lobe lesions in monkeys," by L. Weizkrantz. *2nd Session:* "An experimental study of contour inhibition," by B. Mandell (by invitation). "What is a bar-pressing response?" by J. Trotter (by invitation). "Spontaneous alternation and avoidance of recent stimuli," by N. S. Sutherland (by invitation). "Some experiments on the role of conflict in relation to fatigue," by I. Pleydell-Pearce (by invitation). *3rd Session:* "The analysis of the Limiting Method," by J. Brown and V. R. Cane. "Physiological effects of social relations between field mice," by D. Chitty (by invitation). *4th Session:* "The effect of early exposure to visual patterns on the discrimination of them by adult rats," by E. J. Gibson. "Absolute threshold as a signal/noise problem," by H. Barlow.

10th–11th July, 1956. Extended Meeting at Cambridge. *1st Session:* "The effects of lack of sleep on prolonged watch-keeping," by R. T. Wilkinson (by invitation). "Synaptic morphology in the human cerebrum," by J. R. Smythies (by invitation). "Central inhibition: further observations," by A. Elithorn. *2nd Session:* "Practice and speed in manual skills," by E. R. F. W. Crossman. "The non-randomness of visual threshold determinations," by C. I. Howarth (by invitation). *3rd Session:* "Complex discrimination," by H. M. Bowen (by invitation). "Attitudes in psychophysical investigations," by B. Babington Smith.

26th–27th September, 1956. Extended Meeting at Bristol. *1st Session:* "A theory of shape discrimination," by P. C. Dodwell (by invitation). "Reasoning in rats," by W. S. Anthony (by invitation). "On the relation between response time and response probability," by R. J. Audley (by invitation). *2nd Session:* "Visual perception among the peoples of Malaya," by P. K. Thornton (by invitation). "The effects of television on children," by A. N. Oppenheim (by invitation). *3rd Session:* "Night vision and psychiatric disorder," by G. Granger (by invitation). "At one neuron per discriminable pattern is the brain big enough?" by A. M. Uttley.

2nd–4th January, 1957. 10th Annual General Meeting at Birkbeck College, London. *1st Session:* "Speech production and linguistic restraints," by F. Goldman-Eisler (by invitation). "Parametric models in paired comparison analysis," by M. G. Kendall (by invitation). *2nd Session:* "Compensatory rolling movements of the eye," by P. H. Merton (by invitation). "The effect of food deprivation and stress on the lever-light performance of rats," by H. M. B. Hurwitz and S. C. Dey. "Investigation of the behavioural effects induced by biochemical and neurohumoral agents," by P. H. Glow (by invitation). "Set and interest in a 'perceptual defence' experiment," by B. R. Singer (by invitation). *3rd Session:* "Regional specificity in the organization of the cerebral cortex," by D. A. Sholl (by invitation). "The properties of a mass of cells," by R. L. Beurle (by invitation).

4th Session: "Binaural masking," by J. G. Ingham (by invitation). "The error power spectra of subjects performing a simple task with particular reference to hand tremor," by G. G. Sutton (by invitation).

Committee, 1957.   President: Professor R. C. Oldfield.

Editor: Dr. D. Russell Davis.

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EXPERIMENTAL DISORIENTATION IN THE HORIZONTAL PLANE

BY

MALCOLM PIERCY

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Twenty-eight subjects were examined on a visual matching task for their ability to maintain an orientation with respect to a particular direction in the horizontal plane following a voluntary rotary body movement through 180 degrees. Each subject was examined with respect to eight different directions.

Numerous gross errors occurred when visual information was reduced to the display of an arrow indicating a direction and a second arrow manipulated by the subject. The magnitude and distribution of the errors suggest that, under the conditions of this experiment, visual information as to direction in the horizontal plane is analysed according to the two horizontal dimensions defined by the sagittal and coronal planes of the head. In correcting for the rotary body movement; failure may occur with respect to either or both of these two dimensions. The frequency of a failure to make any correction at all (i.e. 180-degree errors) is consistent with independent failure in each of the two horizontal dimensions.

Failure is markedly more frequent in the fore-aft dimension than in the left-right dimension. It is suggested that this may be explained in terms of the ambiguous spatial significance of vertical disposition on the retina and the possibility of contamination between the two systems of conceptual analysis which identify the vertical and the fore-aft dimensions of visual space.

It is demonstrated that when minimal "landmarks" are provided they tend to be utilized as reference points in attempts to maintain orientation, even when the subject is aware that the "landmarks" are misleading. Such a use of "landmarks" does not suppress the previously mentioned mechanism of dimensional orientation.

The relevance of these results to normal human orientation is discussed.

I

INTRODUCTION

The existence of constancy effects which concern spatial orientation has been fairly generally recognized. Koffka (1935) mentioned that objects in the external world continue to appear upright when the head is tilted. Sherrington (1918) assumed that the phenomenally constant position of the environment during head and eye movements must be achieved by projection of the proprioceptive consequences of these movements into visual perception. Again, Asch and Witkin (1948) point out that the stability of the experience of the upright indicates the presence of a constancy phenomenon analogous to that found in the perception of size, shape and colour. Asch and Witkin have studied orientation in relation to the upright and demonstrated that the structure of the visual field is of primary importance in determining

perception of the upright and that, when visual cues are placed in opposition to tactile, proprioceptive and vestibular cues, orientation in relation to the vertical is predominantly determined by vision. In these experiments Asch and Witkin were studying perception of the vertical in relation to the function of a frame of reference (the structure of the visual field) and in effect demonstrated that constancy of the perceived vertical is largely determined by constant representation of two primary co-ordinates of space (vertical and horizontal).

Under normal conditions perception of the vertical when the head is greatly inclined laterally from the upright position is rarely called for. Moreover, the two main axes into which visual space is usually articulated rarely change in relation to the true vertical, and errors of the type Asch and Witkin describe are likely to occur only when the vertical and horizontal are not implicit in the position and shape of features in the environment (such as walls, trees, the horizon).

It is, however, orientation in the horizontal plane which largely constitutes what is generally understood by spatial orientation; that is to say, the maintenance of a "sense of direction" despite translatory and rotary body movements. Orientation in this plane differs in one important respect from orientation in a vertical plane; there is no unequivocal sensory index of direction corresponding to the vestibular information which (under normal conditions) facilitates perception of the vertical. It is to be expected, therefore, that horizontal orientation will be largely on a visual basis.

A concept which has been found useful by various authors in recent studies of visual constancy effects related to spatial perception is that of a "transformation." Gibson (1954) has pointed out that systematic deformation of the whole visual field during locomotion provides an unequivocal sensory cue as to the kind of locomotion which is occurring. The concept of transformation is also implicit in Langdon's work (1955). He claims that the deformation of a shape during rotation provides axes of orientation which facilitate shape constancy.

This type of theoretical approach may appropriately be applied to the perception of horizontal direction. In small-scale horizontal orientation, constancy of perceived direction is very marked. When a man moves about a room, the room is phenomenally stationary and it is rarely that rapidly executed movements about a vertical axis result in any disturbance of awareness of direction in relation to the immediate environment. Nor is it likely that this ability is due solely to the use of landmarks. A rapid voluntary rotary movement of eyes, head or body rarely results in a visual outlook which is unexpected as to its direction. Conversely, momentary spatial disorientation is not uncommon when one emerges from the London underground railway by a familiar but unintended exit. In navigational terms, "dead reckoning" is at variance with the objectively accurate (but surprising) "fix." It is possible that some such "dead reckoning" (or transformation of visual information) made in accordance with the angle of the eye, head or body rotation assists the "anchoring" of the visual framework for the perception of direction. In the case of rotary movements, the extreme case occurs when a rotary body movement is made through 180 degrees. In this case there is no correspondence between the pre- and post-movement visual fields. Under these conditions, if the environment is an unfamiliar one, directional orientation is perhaps maintained by making some kind of allowance (i.e. a "conceptual" transformation) for the movement which has taken place.

The investigation described below was designed as a preliminary exploration of factors relevant to the constancy of the perception of direction in such small-scale spatial orientation. The experiment is limited to examining the errors in the perception of direction which occur under conditions of reduced visual cues following a voluntary rotary body movement through 180 degrees.

II

Procedure

The subject stood between two 2 ft. 6 in. high tables, placed parallel and 2 feet apart. The tables, which were rectangular, were parallel with the walls of the room. A black cardboard disc was placed flat on the surface of each table and centrally pivoted. On each of the two discs an arrow, 6 inches in length, was painted with luminous paint.

The subject was instructed to note the position of one arrow adjusted by the experimenter, to turn to face the opposite direction and then to adjust the arrow mounted on the other table so that it was parallel with the first arrow and pointing in the same direction. The task was performed in a completely darkened room with the arrows as the only visible objects, but the procedure was first demonstrated to the subject with both arrows in view at the same time and in a normally lighted room. The subject performed examples under these condition to ensure that the nature of the task was fully understood.

The arrow adjusted by the experimenter was, in random order, pointed in the four directions parallel with the sides of the table (described below as cardinal directions) and in the four intercardinal directions: eight directions in all, with intervals of 45 degrees.

This procedure was followed under three different conditions and each of the eight directions was displayed twice under each experimental condition, a total of 48 trials for each subject. Condition 1 consisted of the task as described above with the luminous arrows as the only visible objects. Conditions 2 and 3 were complicated by the presence of a luminous circle immediately ahead of or behind the experimenter's arrow, on the table just beyond the card disc, and a similar circle on the other table. Under condition 2 the circle on the subject's table was placed in relation to the card disc in a position corresponding to that of the circle on the experimenter's table. Under condition 3 the circle on the subject's table was placed in a position opposite to that of the circle on the experimenter's table. This arrangement is illustrated in Figure 1.

FIGURE 1
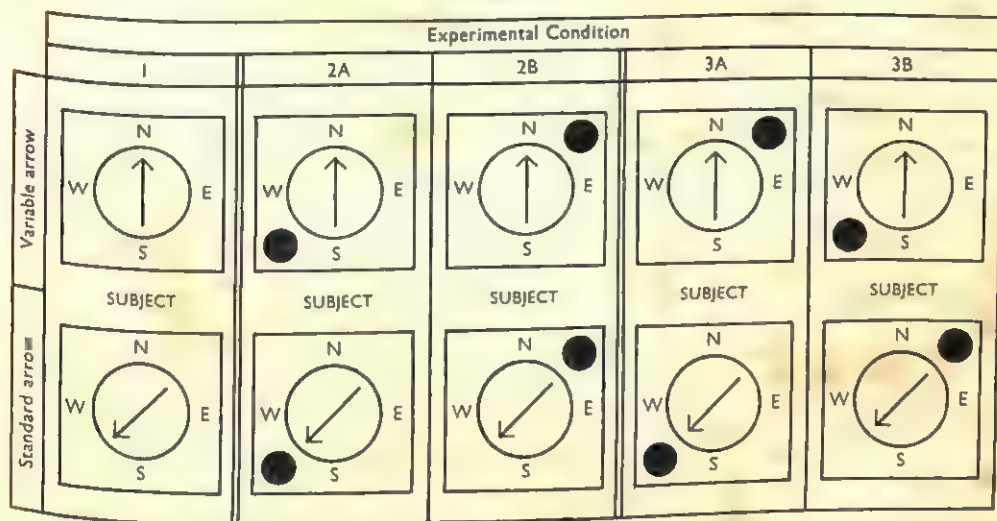
Conditions 2 and 3 did not constitute separate test sequences, but the circles on the two tables were, in random order, in corresponding and opposite positions. The position of the circles thus provided no guide to the correct response. Under both conditions 2 and 3 the circle appeared both ahead of and behind the experimenter's arrow, but in random order. The test sequence constituting conditions 2 and 3 thus consisted of the display (in random order) of each of the eight directions four times: (a) circles corresponding, circle ahead of experimenter's arrow; (b) circles corresponding, circle behind experimenter's arrow; (c) circles opposite, circle ahead of experimenter's arrow; (d) circles opposite, circle behind experimenter's arrow.

Half of the trials constituting condition 1 (circles absent) were carried out before the sequence constituting conditions 2 and 3, and half afterwards. Each subject was instructed

that the task remained the same when the circles were present as when they were absent, namely, to adjust his arrow so that both arrows pointed to the same direction in external space. He was also told that the presence of the circles would not make the task any easier and that the position of the circles would change randomly on successive trials. This was demonstrated with both arrows in view, with normal lighting.

Throughout this experiment the following procedure was adopted in displaying the standard arrow. Initially the subject faced the arrow he was to adjust with this pointing away from him. This direction of the arrow is defined for purposes of reference as "north." On command he then turned through 180 degrees, checking his position by feeling the edges of the table. A screen was then lifted by the experimenter to reveal the standard arrow and replaced after 3 seconds. The subject then turned toward his own arrow, adjusted it and told the experimenter when he was satisfied. On command, the subject then returned the adjustable arrow to "north."

III
SUBJECTS

Twenty-eight subjects were examined, using the technique described above. Sixteen of these were neurotic patients who had been admitted to hospital but who were not undergoing any physical methods of treatment, seven were patients suffering from cerebral lesions, and five were people free from neurological and psychiatric disorder. The experimental procedure was presented to all subjects as a test of their sense of direction. No knowledge of results was permitted during the session. No attempt was made to measure the intelligence of the subjects, but, apart from the neurological patients, all were estimated at interview to be of dull normal intelligence or above.

IV
RESULTS

During a preliminary investigation which preceded the design of this experiment the errors in aligning the arrow were measured in degrees. It was observed, however, that all errors which occurred were either less than 10 degrees, within 10 degrees of 90 degrees, or within 10 degrees of 180 degrees. For the purpose of the present investigation it was therefore decided to study only these large errors which were either about 90 degrees or about 180 degrees. All errors which exceeded 10 degrees were of these two types.

TABLE I
TOTAL ERROR INCIDENCE (28 SUBJECTS)
Direction of Standard Arrow

	N	S	E	W	N-E	S-E	N-W	S-W	Total for 8 directions
Total Errors Made ..	52	52	19	14	56	58	56	50	357 (26·6%)
Total Possible Errors..	168	168	168	168	168	168	168	168	1,344 (100%)

By this criterion 22 of the 28 subjects made at least one error and the mean number of errors per subject for the group of 28 subjects was 13, the maximum possible number of errors for each subject being 48. The only essential differences between the performances of the normal, the psychiatric and the neurological subjects were differences in the total number of errors made. The mean number of errors per subject was 11 for the normal subjects, 10·5 for the neurotic subjects and 18·9 for the neurological subjects. The type of error was (with the exception of two neurological subjects discussed below) closely similar in all three types of subject and the results reported

below refer, unless otherwise stated, to the totalled results of the 22 subjects who made at least one error. The total error incidence for each of the eight direction is shown in Table I.

Errors on Cardinal Direction

A total of 137 errors was made by 17 subjects on the cardinal directions. The total possible number of errors for all 28 subjects was 672 and the error incidence for these subjects was thus 20·4 per cent. With the exception of two errors made by a patient suffering from a Korsakoff type dementia, all these errors were errors of 180 degrees. Forty-one of these errors were made under experimental condition 1, when no circles were present. The subject who made the two anomalous errors (both 90-degree errors) had a severe defect of immediate memory and it is probable that these two errors resulted directly from this defect rather than from the systematic error tendency exhibited by other subjects.

The tendency for errors to occur was very much greater when the standard arrow pointed immediately towards or away from the subject than when it pointed immediately to the subject's left or right. Under experimental condition 1, 33 errors occurred when the standard arrow pointed in the directions defined above as north and south but only 6 errors occurred when the arrow pointed east and west. A student's "t" test established that this difference in the incidence of errors was significant ($t=3·90$; $p <·001$). Under experimental condition 2, the errors on north and south totalled 25 and those on east and west 7 ($t = 2·81$; $p < ·02$); and under the third experimental condition, the errors on north and south were 46 and on east and west 18 ($t = 3·52$; $p < 0·01$). With the exception mentioned above, these errors were all errors of 180 degrees; that is to say that, in adjusting the variable arrow, the subject fails to correct for the fact that he has turned through 180 degrees since inspecting the standard arrow.

Errors on Intercardinal Directions
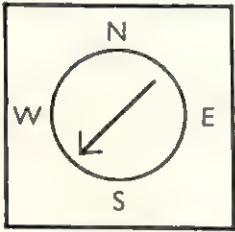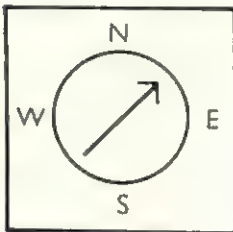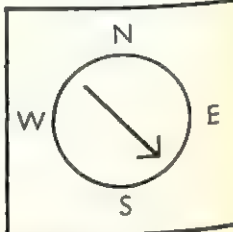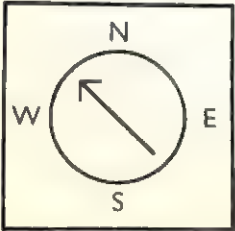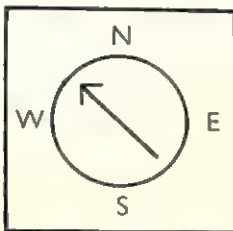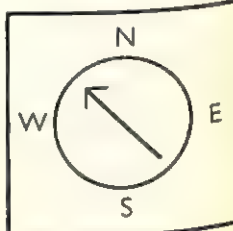
Under the first experimental condition (circles absent), 18 subjects made a total of 79 errors on the intercardinal directions. The total possible number of errors for all 28 subjects under experimental condition 1 was 224, and the mean error incidence for these subjects was thus 35·3 per cent. The magnitude of 23 of these 79 errors was (like the errors on the cardinal directions) within 10 degrees of 180 degrees. However, in the case of the remaining 56 errors, the error was within 10 degrees of 90 degrees. All subjects who made any errors on the intercardinal directions made at least one error of 90 degrees and 16 of these 18 subjects made more 90 degree errors than 180 degree errors. Under experimental conditions 2 and 3 (circles present) errors on intercardinal directions were also either 90 degrees or 180 degrees. 49 of the 63 errors occurring under experimental condition 2 were 90 degree errors, as were 41 of the 85 errors occurring under condition 3.

The fact that only 180 degree errors occurred on cardinal directions and that errors on intercardinal directions were either 90 degrees or 180 degrees suggested the possibility that errors on the intercardinal directions were made with respect to two independent dimensions. The hypothesis was formed that when the standard arrow pointed to an intercardinal direction, the correct adjustment of the variable arrow was achieved by organizing the visual information (position of standard arrow) in terms of the two dimensions corresponding to the positions north and south on the one hand and east and west on the other. Separate transformations through 180 degrees of the information relating to these two dimensions might then be made to correct for the bodily movement through 180 degrees. Thus when an error occurred

it might be a failure to make a transformation in the north-south dimension, a failure to make a transformation in the east-west dimension, or a failure to make either transformation. The first two types of failure would produce 90 degree errors and the third type would produce 180 degree errors.

The 90 degree errors on the intercardinal directions were therefore classified as north-south failures or east-west failures, with a view to examining whether their comparative incidence corresponded to that observed in the case of errors on the cardinal directions. The three kinds of error which occurred in this experiment are shown separately for cardinal and intercardinal directions in Figure 2. The results

FIGURE 2

of the classification of the 90 degree errors and also the incidence of 180 degree errors are shown in Table II. Under experimental condition 1, 41 of the 90 degree errors were north-south failures and 15 were east-west failures. This difference is statistically significant ($t = 2.16$, $p < 0.05$). Under condition 2, north-south and east-west failures were respectively 43 and 6 ($t = 3.69$, $p < 0.01$) and under condition 3 they were 38 and 3 ($t = 3.33$, $p < 0.01$). Thus it is apparent that the excess of north-south over east-west errors which was observed in the case of the cardinal directions has its counterpart in the 90 degree errors occurring on the intercardinal directions.

The incidence of 180 degree errors on the intercardinal directions appears to be consistent with the hypothesis that they are constituted by random coincidence of two independent sources of error, namely, north-south transformation failure and east-west transformation failure. The totalled figures for the 28 subjects cannot be used as the basis for the calculation of the expected coincidence of the two types of error, since error incidence varies in different subjects. Also the actual number of errors made by individual subjects are too small for independent calculations (e.g. X^2)

to be made for each subject. It is, however, possible to inspect the incidence of the three types of error in each subject and determine, on the hypothesis of two independent sources of error, the expected ratio between the number of combined errors (i.e. 180 degree errors) and the number of single east-west errors. This expected ratio

TABLE II
Type of Error Occurring under Three Experimental Conditions

	Experimental Condition 1			Experimental Condition 2			Experimental Condition 3		
A									
B	90°	180°		90°	180°		90°	180°	
C		N-S	E-W		N-S	E-W		N-S	E-W
Errors on Cardinal Directions	2	33	6	0	25	7	0	46	18
C	N-S	E-W		N-S	E-W		N-S	E-W	
Errors on Intercardinal Directions	41	15	23	43	6	14	38	3	44
C	N-S	E-W		N-S	E-W		N-S	E-W	
Total Errors involving 1 dimension only	74	21		68	13		84	21	

The distribution of errors is shown according to A, the experimental condition involved, B, the magnitude of the error and C, the dimension in which a transformation failure occurs. The total errors refer to the sum of 90° errors on the intercardinal directions and 180° errors on the cardinal directions.

may then be compared with the observed ratio. Table III shows such an analysis for the intercardinal errors made by each subject under experimental condition 1. The column headed "expected preponderance of combined errors" shows with a plus sign that the chance of the combined errors exceeding the single east-west errors is greater than 50 per cent., with a minus sign that such a chance is less than 50 per cent. and with a zero that such a chance is 50 per cent. It can be seen that in 9 cases out of the 18 a preponderance of combined errors would be expected, that in 6 cases a preponderance of east-west errors would be expected and that in 3 cases the two types of error would be expected to be equal in number. In only one case was there a discrepancy between the observed and the expected preponderance of errors. In this case the four errors involving an east-west failure which occurred would be expected to be distributed between the combined and the single errors in the ratio 3:1. In point of fact they are distributed equally.

The results under experimental conditions 2 and 3 cannot be inspected in this way since the presence of the circles introduces a fresh source of error.

The Effect of the Datum Circles

When circles were used they were placed on the two tables either in corresponding positions, so that using the circle as a "landmark" resulted in a correct response (condition 2), or in opposite positions, so that using the circle as a "landmark" resulted

in a 180 degree error (condition 3). In so far as the circle is used as a point of reference, one may expect a decrease in the number of 180 degree errors under condition 2 and an increase in the number of these errors under condition 3.

TABLE III

ERRORS OCCURRING ON THE INTERCARDINAL DIRECTIONS UNDER EXPERIMENTAL
CONDITION I (18 SUBJECTS)

N-S Errors	E-W Errors	Combined Errors	Observed	Expected	
3	0	5	+	+	
0	1	0	−	−	
7	0	1	+	+	
4	2	2	0	+	
1	1	0	−	−	*Observed* = observed preponderance of combined errors over E-W errors.
0	1	0	−	−	
5	0	1	+	+	
3	0	0	0	0	
5	0	1	+	+	
1	0	0	0	0	*Expected* = expected preponderance of combined errors over E-W errors.
0	1	0	−	−	
5	1	2	+	+	
1	0	0	0	0	
1	1	4	+	+	
2	1	3	+	+	
0	4	2	−	−	
0	2	0	−	−	
3	0	2	+	+	

The column headed "Expected" shows with a "+" that, on the hypothesis that combined errors result from random coincidence of N-S and E-W errors, the chance of combined errors exceeding single E-W errors is greater than 50 per cent, with a "−" that such a chance is less than 50 per cent. and with a "o" that such a chance is 50 per cent. In the column headed "Observed," a "+" signifies that there are more combined errors than single E-W, a "−" that there are fewer combined than single E-W errors and a "o" that the numbers of the two kinds of error are equal.

(1) *Intercardinal errors.* The effect of the presence of the datum circles is to decrease the number of 180 degree errors on the intercardinal directions under condition 2 and to increase the number under condition 3. Whereas 23 180 degree

errors occur on the intercardinal directions under condition 1, only 14 occur under condition 2, and as many as 44 under condition 3 (Table II). A "t" test shows the difference between the mean 180 degree error incidence on intercardinals under condition 3 to be significantly greater than the corresponding error incidence under condition 2 ($t = 2.46$, $p < 0.05$). Clearly the circles are to some extent used as points of reference and the effect of this tendency on the incidence of errors resulting from a failure to make a transformation in one only of the two dimensions discussed above may now be considered.

Under condition 1 there are 15 90 degree errors attributable to failures of left-right transformation; under condition 2 there are only 6 such failures and under condition 3 there are only 3. A "t" test carried out on the errors of all subjects making left-right transformation failures shows the incidence of left-right failures to be significantly lower when the datum circles are present (conditions 2 and 3) than when they are absent (condition 1) ($t = 3.18$, $p < 0.01$).

In the case of 90 degree errors attributable to failure of fore-aft transformation, the situation is quite different. Under condition 1, 41 such errors occur; under condition 2, 43 and under condition 3, 38. Thus the use of the datum circles as points of reference has no observable effect on the incidence of 90 degree errors arising from failure to make a transformation in the fore-aft dimension.

(2) *Cardinal errors.* In the case of the cardinal directions also there is clear evidence that the datum circles are used as "landmarks" in orienting the variable arrow. When the circles are in corresponding positions on the two tables, 25 errors occur on north and south and 7 errors on east and west; when the circles are in opposite positions on the two tables, 46 errors occur on north and south and 18 on east and west. When, in the case of each subject, the errors occurring under condition 2 are subtracted from those occurring under condition 3, the mean of these differences is found on a "t" test to be significantly greater than zero ($t = 2.76$, $p < 0.02$).

(3) *Anomalous effect of datum circles.* The performance of one subject was influenced by the presence of the datum circles in an unusual manner. This subject was a patient suffering from a lesion of the left frontal lobe and exhibiting mild dementia. He showed a very strong tendency to point the variable arrow directly towards the juxtaposed circle, irrespective of whether this was a correct response and irrespective of whether the arrow on the other table pointed towards or away from its own circle. That is to say that, unlike other subjects, he oriented the arrow not with reference to the position of the circle but towards the circle. Conditions 2 and 3 consisted of a total of 32 trials, 16 of which would result in a correct response if the subject pointed the arrow directly towards the circle. This subject pointed the arrow directly towards the circle on 30 of the 32 occasions he was tested. He was repeatedly questioned during the test session as to the nature of the task required of him and he always appeared to understand what was required. He was also able to perform the task correctly when the room was fully lit and both tables were in view at the same time.

V

DISCUSSION

Three main conclusions emerge from these results:—

(1) following a voluntary movement through 180 degrees, it is more difficult for a subject to correct for directional visual information relating to the fore-aft dimension than to correct for similar information relating to the left-right dimension;

(2) in making such a correction under the conditions of this experiment, errors occur as though independent corrections are made with respect to the two horizontal dimensions;

(3) if landmarks are provided they tend to be utilized, but this tendency by no means obliterates the previously mentioned tendency to make separate corrections for the position of the arrow in relation to the two horizontal dimensions of space.

An explanation for the greater difficulty in making a transformation in the fore-aft dimension as compared with the left-right dimension may be sought by considering some of the characteristics of the visual stimuli involved.

Provided that the head is not greatly tilted laterally out of the sagittal plane, the left-right dimension on the retina always corresponds unambiguously to the right-left dimension in external space. The vertical dimension on the retina is, however, ambiguous. Two points vertically disposed on the retina may correspond to relative positions in external space either in the vertical dimension or in the fore-aft dimension or in both of these dimensions. Moreover, under certain conditions the retinal correlate of the external vertical dimension is reversed in sign. This is the case when one looks obliquely downwards at a surface sloping downwards and away from the eye (or obliquely upwards at a surface sloping upwards and away from the eye). It follows, therefore, that when a voluntary movement through 180 degrees is executed, information deriving from the left-right dimension on the retina must always be transformed through 180 degrees if orientation is to be maintained. However, in the case of information derived from the vertical dimension on the retina, the transformation is necessary if the retinal dimension refers to the fore-aft dimension in space but not (usually) if it refers to the vertical dimension. Thus whether or not a transformation of the retinal vertical is required following a rotary movement of the body will depend upon further visual information.

One need only suppose that there is some contaminatory effect between the mechanisms of visual orientation with respect to the vertical and those with respect to the fore-aft dimension for a preponderance of fore-aft over left-right errors to be expected in this experiment. If there is a tendency to respond to the arrows "as though" they were lying in a vertical plane, then, when this tendency operated, no transformation of information relating to the fore-aft dimension would be made following a rotary body movement through 180 degrees. Presumably the condition of reduced visual cues facilitates the effect in a way which very rarely occurs in every-day life.

Similar contaminatory effects have been observed before. Elithorn and Piercy (1953) reported that errors of tactile localization on the fingers suggested that the subject decided separately which finger and which side of a finger was stimulated, and claimed that a subject tended to be influenced in deciding which side of a finger was stimulated by the side of the hand which was stimulated and *vice versa*. Reid (1954) demonstrated an illusion of hand movement which compensated for the visual horizontal-vertical illusion and he observed that left-right movements were underestimated in relation both to vertical movements and to horizontal movements towards and away from the body. In this situation the vertical and fore-aft dimensions were therefore in some sense equivalent.

Reid suggests that the visual horizontal-vertical illusion is learned and secondary to the illusion of movement. In view of the present experimental results, the "perspective theory" is worth reconsidering. The image on the retina of a line oriented in the fore-aft dimension with respect to the head will be more or less foreshortened according to the height of the eye above the line. In estimating distances, the constancy effects make it possible for this foreshortening largely to be allowed for. If

there is any tendency to make a similar but inappropriate allowance for images which fall vertically on the retina but which refer not to the fore-aft dimension in the external world but to the vertical, then some over-estimation of vertical as compared with horizontal (left-right) lines is to be expected.

All subjects in the present experiment were quite aware that the arrows were flat on the table; they handled them and saw them in a fully lighted room before the experiment began and continued to handle one of the arrows throughout the experiment. The confusion which occurs is not therefore a global disorientation such as was elicited by Asch and Witkin's procedure. Subjects were not globally disoriented in the room; they were merely unable to maintain a stable directional orientation when this was tested in terms of conventional symbols. The confusion (or equivalence) between the visual vertical and fore-aft dimensions in this experiment obtrudes itself not in judgements as to verticality or horizontality but in more complex orientation judgements where horizontality or verticality is "assumed" but not independently "judged." If such confusion between vertical and fore-aft is in fact occurring, it occurs despite previous experience of the arrows, despite continuous tactile information, despite cues from arrow movement and despite the ability of the subject to report that the arrow is flat on the table. Nevertheless, the arrows are not accurately perceived at a schematic or symbolic level (this experiment) unless extra visual information is provided (fully lighted room). There would appear to be some error, facilitated by the ambiguity of vertical disposition on the retina, in the preliminary classification of information which presumably occurs before this information is used in conjunction with other information to arrive at a final solution.

One further possibility may be considered in relation to this vertical-fore-aft confusion. It is possible that, where there is little or no redundancy of visual information, intra-modal ambiguity may be much more difficult to resolve accurately than inter-modal ambiguity (the sensory rivalry type of experiment). Hence in this experiment redundancy of specifically visual information would be at a premium, even though adequate relevant information from other modalities were present.

The foregoing discussion has suggested some equivalence between visual perception of the vertical and of the fore-aft dimensions. This is in contrast to the independent reference to the left-right and fore-aft dimensions which appears to be made in the perception of direction. This choice of the fore-aft and left-right co-ordinates as independent dimensions of reference could be either a general principle of human orientation or else an effect which occurs simply because it is specifically favoured by the conditions of the experiment. The balance of previous experimental work would seem to favour the latter hypothesis and to suggest that people tend to utilize whatever co-ordinates or landmarks are available. There may nevertheless be particular dimensional structures which are preferred. One of Koffka's (1935) experiments for example showed that localization of a sound (using the centring technique) was very precise if a subject was seated squarely facing one of the walls of the room, but was comparatively inaccurate if the subject was seated facing at an angle to the main horizontal axes of the room. This inaccuracy persisted even when the subject closed his eyes. Similarly, Asch and Witkin showed that sometimes when their experimental room and/or chair was being progressivly tilted laterally from the upright position a subject might claim that the room was still vertical until, quite abruptly, he would claim that the room was lying horizontally, or else lying on one edge like a diamond. It seemed that errors in such subjects occurred because they tended either to respect the conventional reference dimensions provided by the shape of the room, or, when incompatible information from other sense modalities became too compelling, to abruptly rotate the axes of the visual dimensions being utilized through either 90 or

45 degrees and to suppose that the room was lying on its side or else standing on one edge. Thus the room continued to be perceived as symmetrically oriented in relation to the vertical. This intolerance of two perceived systems of co-ordinates not bearing a simple relationship to each other is closely analogous to Koffka's result.

In the present experiment the two tables were placed with their sides parallel to the walls of the room, so there was no incompatibility between personal left-right and fore-aft co-ordinates and the co-ordinates defined by the four walls of the room. The significance of the co-ordinates used by the subject cannot therefore be fully assessed without further experimentation (e.g. with the subject having no knowledge of his orientation in the room).

When the field of vision is complicated by the presence of a luminous circle close to each arrow, this circle tends to be used as a point of reference. It is noteworthy that this use of landmarks appeared to be "forced" and largely independent of the subjects' insight. All subjects were able to report that using the circles as landmarks was as likely to result in a 180 degree error as in a correct response. Some subjects reported that the circles were confusing, even when a deliberate attempt was made to ignore them. Others claimed that the presence of the circles made no difference to the difficulty or nature of the task, but nevertheless produced results pointing quite definitely to systematic orientation in relation to the position of the circles. Throughout this experiment there was a remarkable absence of relationship between subjective assessment of performance and the number and character of errors. There was no apparent relationship between subjective confidence and objective accuracy, both of which varied widely and, although there was striking uniformity in the type of errors made by different subjects, many different methods of doing the task were reported. Some verbalized the task throughout and gained no obvious benefit from this, others claimed to maintain some kind of orientation in relation to a particular wall of the room (which they could not see) and still others felt quite unable to rationalize their performance.

However, when the circles were present, errors attributable to transformation failures continued to occur independently of errors attributable to using the circles as landmarks. This was clearly happening when the standard arrow was pointing to the intercardinal points. Although the presence of the circles caused a decrease in 180 degree errors under condition 2 and an increase under condition 3, nevertheless 90 degree errors continued to occur in the same subjects and north-south failures were greatly in excess of east-west failures. These 90 degree errors are quite inconsistent with using the circles as landmarks and it may be supposed that when these two orientation tendencies were incompatible one or the other predominated and that this predominance varied from trial to trial in the same subject.

Under natural conditions in a familiar environment, orientation is probably usually experienced as a sequence of adjustments to landmarks, these being continuously abundant. However, introspective report on orientation is demonstrably unreliable and something akin to "dead reckoning" navigation very probably plays an important rôle.

Finally, the performance of the subject who always pointed the arrow towards the circle should be mentioned. Such orientation towards rather than in relation to a point of reference might suggest that the nature of the task was not understood. This possibility was, however, excluded. It may therefore be that in this subject (assuming that his performance was not specific to the cerebral lesion which was present in this case), the complexity of the task forced a reversion to a very primitive type of orientation not normally seen in human behaviour.

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THE TIMING OF BAR-PRESSING BEHAVIOUR

BY

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The time spent by a rat in a bar-pressing situation is made up of active time spent n pressing, eating time, and extra time spent in other activities. With a well trained rat, active time and extra time are small, and eating time mainly determines the rate of reward delivery. Active time is affected by a change of weight on the bar, the time between reward deliveries is affected by the amount of reward, and the extra time is affected by extinction conditions.

There is not a one-to-one correspondence between periods of activity at the knob and rewards.

The term "response" and some variables based on it are given empirical referents, which show that much research and theorizing on bar-pressing behaviour has been concerned with only a small selection of the rat's bar-pressing activities. Some reasons for this restriction are the use of the simple weighted bar, the lack of a rationale for bar-pressing research, and the practice of not watching the rat during an experiment.

INTRODUCTION

This paper is concerned with the durations of the various kinds of behaviour, pressing, eating, etc., which rats perform in a weighted bar-pressing situation. The common method of describing and measuring such behaviour is discussed in the light of new principles and experimental results. Understanding of this paper will be aided by reference to the writer's earlier papers (Trotter, 1956*a* and 1956*b*).

APPARATUS AND PROCEDURE

The subjects were 18 male hooded rats aged about three months at the beginning of the experiment. The cage was 12 in. long, 7 in. broad and 10 in. high, and had a front wall made of perspex, the remainder of the wall being formed from a sheet of aluminium bent to a U-shape, the whole mounted on a grill floor. On one limb of the U-shaped wall was the feeding trough, and in the other was a window through which the knob of the bar-pressing recorder could be inserted. An observer looking through the perspex wall could see the rat facing him, turning to its right to press the knob, and to its left to pick up food from the trough. The rewards were pieces of spaghetti (usually ¼ in.), delivered by the Deutsch spaghetti gun (1956*a*) when the rat pressed the knob right down (about 1 cm.). The bar-pressing recorder was an early version of that described in (1956*b*). It provided, inked on kymograph paper moving at 1·25 cm. per second, a temporal record of the force applied vertically to the knob, and of delivery of reward, but no record of displacement.

The rats were used in random order each day, usually with 21–23 hours' food deprivation, and were fed together in the home cages for one hour after each day's experimenting. The experimental programme is shown in table I.

Initial training was just sufficient to teach the rats to press the knob weighted with 5 gm. The training with increasing weights repeated the procedure used in a previous experiment (1956*a*), using now a weighted bar instead of a sprung bar, but with number of rewards daily an experimental variable. Experimental trials were introduced at intervals in a long series of daily practice trials in the standard situation:—50 gm. on the bar; ¼ in. of spaghetti for reward; 21–23 hours' food deprivation. The experimental variations from the standard were the following. On day 20 two groups of three rats were given a trial commencing with the unusual weight of 70 gm. or 20 gm., and after 20 rewards reverting to the usual 50 gm. for 10 rewards. Another group of three rats had a trial with 50 gm. as usual. On day 22 the knob was removed and the window was covered up. The rat was placed in the cage, with one ¼ in. piece of spaghetti in the trough. As soon as the rat had eaten that piece and put its nose back into the trough, another piece was delivered and so on for 15 pieces. This procedure established for each rat the average time needed

to pick up and eat the ¼ in. of spaghetti. On day 25 three groups of six rats were given trials with ⅛ in., ¼ in. and ½ in. pieces of spaghetti. Between days 31 and 35 groups of three rats were given trials after different periods of food deprivation—0, 0, 5, 11, 23 and 47 hours. On the last day the rats were given an extinction trial of about 10 minutes. Samples of well practised behaviour in the standard situation came from days 19, 24, 30 and 38 for all rats, and some from days 20 and 31–35 for a few rats.

TABLE I

THE EXPERIMENTAL PROGRAMME

Day No.		1–6	7 and 8	9	10	11	12	13	14–18	19
Remarks	initial training	practice	sample	training with increasing weight					practice	sample
Weight (gm.)	5	5	5	10	20	30	40	50	50	50
No. of rats ..	18	18	18	6	6	6	similar		18	18
No. of rewards	until trained	15	15	5	10	20	similar		10	15
Records ..	—	—	X	X	X	X	X	X	X	—	X

Day No.	20			21	22	22 and 23	24	25		
Remarks	unusual weights			practice	eating	practice	sample	different rewards		
Weight (gm.)	20–50	50	70–50	50	—	50	50	50		
No. of rats ..	3	3	3	18	18	18	18	6	6	6
No. of rewards	20–10	20	20–10	10	15	10	12	10 ⅛ in.	10 ¼ in.	10 ½ in.
Records ..	X	X	X	—	X	—	X	X	X	X

Day No.	26–29	30	31–35						36 and 37	38	39
Remarks	practice	sample	hours of hunger						practice	sample	extinction
			0	0	5	11	23	47			
Weight (gm.)	50	50	50						50	50	50
No. of rats ..	18	18	3	3	3	3	3	3	18	18	9
No. of rewards	12	12	12						10	10	10 min.
Records ..	—	X	X	X	X	X	X	X	—	X	X

PRINCIPLES OF THE BAR-PRESSING METHOD

The record in Figure 1 illustrates the following principles. These principles apply to the simple weighted bar, but this does not provide the information necessary for their proper use.

Sample records

Excerpts from trials in the standard situation:—

    (a) a non-repeating rat;
    (b) a repeating rat.

Engagements from extinction trials:—

   (c)   a non-repeating rat;
   (d)   a repeating rat.
   (e)   commencement of a trial showing the lag of the first contact
         after the first engagement.

Numerals in the upper line indicate the durations of intervals in seconds.
Numerals in the lower line indicate zero interval groups of contacts.

FIGURE 1
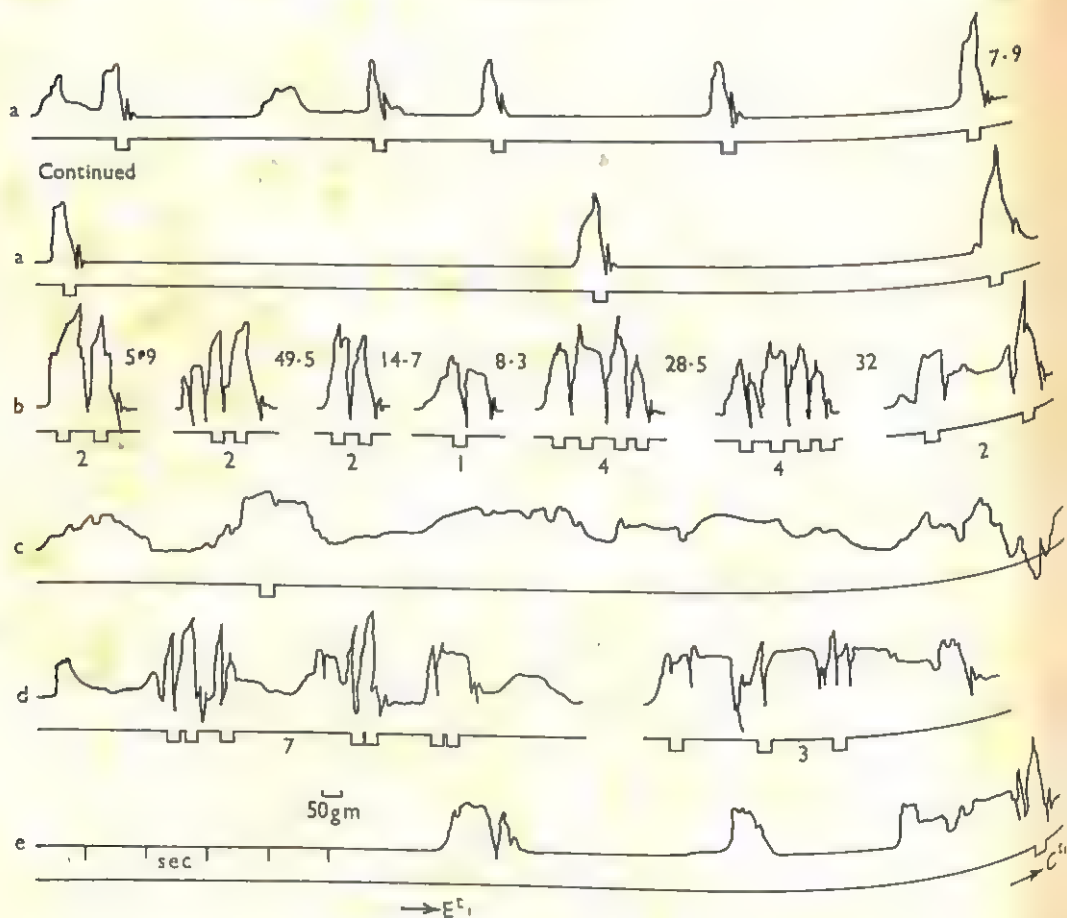
Events

An engagement is a period during which the rat is touching the knob.

An interval is a period between two consecutive engagements.

A contact occurs the first time the knob is moved to the lower stop after coming from the upper stop, since the apparatus is single-acting, and the knob must go to the upper stop to reset it.

An operation is an activation of the food delivery mechanism. It can be initiated by a contact.

A reward may be produced by an operation when the apparatus is so adjusted.

A sound is made by the mechanism on each operation.

Number of events

Numbers of events are denoted by the capital initial letters of their names:—E, I, C, O, R, S.

Durations of sorts of behaviour

Active time when the rat is touching the knob (t_a sec.).
Off time when the rat is not touching the knob (t_o).
Eating time spent in picking up and eating food (t_e).
Extra time spent in washing, resting, exploring, etc., or any other activity not specifically denoted (t_x).

By definition, between any two specified instants:—

$$\text{total time} \equiv t_a + t_o$$
$$t_o \equiv t_e + t_x$$
$$\text{total time} \equiv t_a + t_e + t_x.$$

These are general symbols referring to the variables *per se*, and not to any particular periods. Specific durations are indicated thus:—

Active time of a particular engagement ($_E t_a$).
Off time of a particular interval ($_I t_o$).

Average durations are indicated thus:—
Average active time per contact ($_C \bar{t}_a = \Sigma_C t_a / C$).
Average off time per reward ($_R \bar{t}_o = \Sigma_R t_o / R$).

Times of first engagement and of first contact

The times elapsing from the beginning of a trial to the first engagement and to the first contact are denoted by $_E t_1$ and $_C t_1$. The first by definition consists of a single period of off time. The latter may include a number of periods of active and off time.

Situation, output and result

The experimenter determines the conditions of a contact by (a) fitting a suitable weight (W gm.) to the bar, and (b) adjusting the stops to allow a suitable maximal displacement (s cm.) of the knob. He also determines whether a contact operates the mechanism, whether an operation produces a reward, how many contacts are required for a reward, the delay of reward, etc.

The rat produces an output describable in terms of time, force and impulse (1956a), and number of engagements.

In a given situation a given output produces a result describable in terms of displacement of the knob and numbers of events—contacts, rewards, etc.

RESULTS

Well practised behaviour

The samples of well practised behaviour in the standard situation amounted to a grand total of 1,231 engagements and 1,203 rewards, giving a rewards-per-engagement figure (R/E) of 0.98. This average value by no means indicates a tendency to a one-to-one correspondence between engagements and rewards. Sometimes the rat failed to move the knob, and sometimes it failed to press the knob right down, and no reward was delivered. The commonest engagement was one producing a single reward. Often a series of rewards were produced in succession in a single engagement. This behaviour is called repeating at zero interval, and it results in a zero interval group of contacts. Table II gives the complete account of engagements and rewards, arranged in order of R/E.

The averages per reward of active time ($_R \bar{t}_a$) and off time ($_R \bar{t}_o$) for these trials are given in table III, together with average eating time per ¼ in. reward ($_R \bar{t}_e$) as found on day 22. For any rat, individual off times and eating times varied a great deal, since as many as six or more rewards might be eaten hurriedly, but inevitably a pause was required while the rat cleaned its teeth of the spaghetti fragments.

TABLE II
Engagements and Rewards in Well-practised Behaviour

Rewards per engagement ..	1·98	1·23	1·18	1·11	1·06	1·03	1·02	1·0	0·94	0·92
Rewards ..	97	90	66	50	66	70	50	49	64	86
Engagements ..	49	73	56	45	62	68	49	49	68	93
0 rewards ..	2	8	4	5	6	13	7	4	10	17
1 ,, ..	16	44	40	32	48	42	34	41	53	67
2 ,, ..	18	17	10	6	7	11	8	4	4	8
3 ,, ..	9	4	2	2	—	2	—	—	1	1
4 ,, ..	2	—	—	—	1	—	—	—	—	—
5 ,, ..	2	—	—	—	—	—	—	—	—	—

Rewards per engagement ..	0·91	0·89	0·87	0·83	0·83	0·79	0·78	0·73	Total 0·98
Rewards ..	64	70	84	49	64	69	49	64	1,203
Engagements ..	70	79	96	59	77	89	61	88	1,231
0 rewards ..	13	14	16	10	19	23	14	29	214
1 ,, ..	52	60	76	49	53	63	45	54	869
2 ,, ..	3	5	4	—	4	3	2	5	119
3 ,, ..	2	—	—	—	1	—	—	—	24
4 ,, ..	—	—	—	—	—	—	—	—	3
5 ,, ..	—	—	—	—	—	—	—	—	2

TABLE III
Active Time, Off Time and Eating Time in well-practised Behaviour

Day											
Day 19	$_R\bar{l}_a$ ..	1·0	1·0	1·4	1·1	1·4	1·2	0·9	1·2	1·2	0·8
	$_R\bar{l}_o$ ..	6·2	9·1	8·0	7·5	11·5	7·5	10·8	14·1	9·1	11·6
Day 24	$_R\bar{l}_a$ ..	0·8	0·8	1·4	0·8	1·5	1·4	1·0	1·3	1·0	0·9
	$_R\bar{l}_o$ ..	6·0	8·2	7·5	7·0	14·8	6·5	11·8	14·0	7·1	9·9
Day 30	$_R\bar{l}_a$ ..	1·1	1·4	0·8	0·7	1·0	1·4	1·8	1·0	1·0	0·7
	$_R\bar{l}_o$ ..	6·7	19·0	4·6	8·9	7·3	8·7	10·2	12·1	8·9	8·3
Day 38	$_R\bar{l}_a$ ..	0·8	1·3	0·6	1·0	1·2	1·3	1·0	1·0	1·0	1·2
	$_R\bar{l}_o$ ..	7·7	4·8	5·9	7·6	9·8	4·7	9·2	14·6	9·6	7·4
Day 22	$_R\bar{l}_e$ ..	6·1	6·6	7·5	8·6	7·4	5·7	9·3	13·7	6·8	8·1

Day											Mean
Day 19	$_R\bar{l}_a$ ..	0·9	1·0	1·3	1·0	1·5	1·4	1·2	1·1		1·2
	$_R\bar{l}_o$ ..	7·2	5·9	6·0	7·2	15·1	8·0	16·4	12·6		9·7
Day 24	$_R\bar{l}_a$ ..	1·0	0·8	0·7	1·2	0·8	1·4	1·0	1·3		1·1
	$_R\bar{l}_o$ ..	8·2	7·5	6·2	7·8	8·9	7·5	14·6	17·5		9·6
Day 30	$_R\bar{l}_a$ ..	1·3	1·2	0·7	1·1	0·6	1·0	0·9	1·0		1·1
	$_R\bar{l}_o$ ..	11·1	6·7	5·6	6·3	7·0	8·3	9·4	12·5		8·9
Day 38	$_R\bar{l}_a$.	0·8	1·4	0·9	2·0	1·2	1·0	0·9	1·8		1·1
	$_R\bar{l}_o$ ..	7·7	6·8	5·4	5·6	6·8	4·7	11·0	9·8		7·6
Day 22	$_R\bar{l}_e$ ..	7·5	5·8	6·8	7·4	7·2	6·9	11·2	10·8		7·95

Overall means $_R\bar{l}_a = 1\cdot1$ $_R\bar{l}_o = 8\cdot95$

Figure 2 shows the scatter diagram and the regression line for $_R\bar{l}_e$ and $_R\bar{l}_o$. With a correlation coefficient $r = 0\cdot62$ ($p < 0.001$), the regression equation is:—

$$_R\bar{l}_o = 1\cdot6 + 0\cdot93_R\bar{l}_e.$$

The coefficient 0·93 is within sampling error of 1·0, indicating that the average off time per reward is about 1 sec. longer than the average eating time. The extra time of 1 sec. shows that the rat, when well trained, does little else but press and eat. This conclusion was stated without detailed proof in an earlier paper (1956a).

FIGURE 2
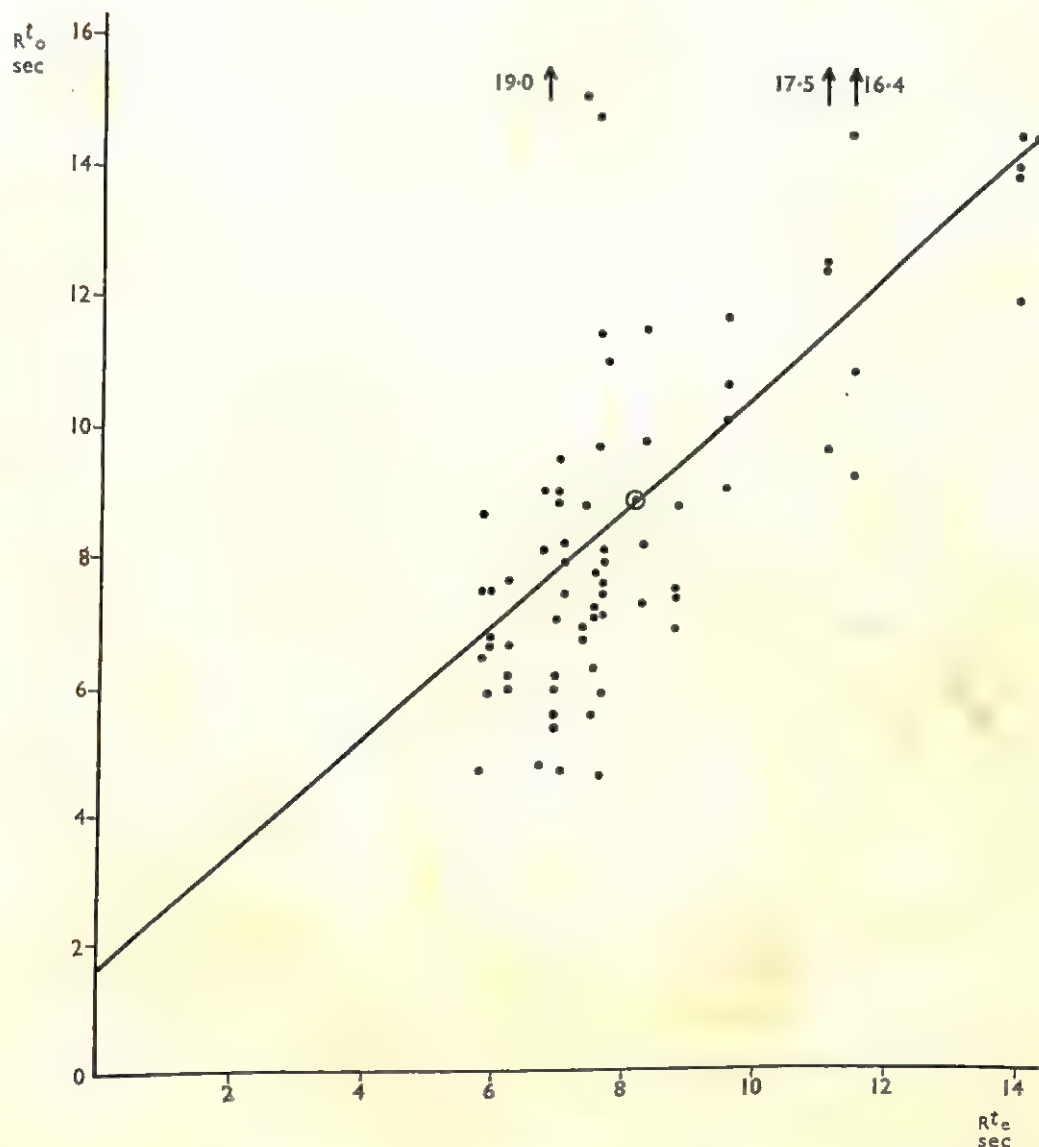
The relation between eating time and off time.

Using the mean point, circled in Figure 2, the times mentioned in the previous section are found to be:—

$$\text{Average total time per reward} = {}_R\bar{t}_o + {}_R\bar{t}_e + {}_R\bar{t}_x$$
$$10\cdot05 = 1\cdot1 + 7\cdot95 + 1\cdot0.$$

In most trials the first contact was not produced by the first engagement. In general $_c t_1$ exceeded $_E t_1$. For 18 rats on four trials each the average numbers of engagements completed before the first contact (not counting the engagement producing the first contact), and the average times separating the beginning of the first engagement and the first contact are shown in table IV, which also shows a frequency distribution of the numbers of engagements preceding the first contact.

TABLE IV

ENGAGEMENTS COMPLETED BEFORE FIRST CONTACT

Averages for each rat on four trials

E $_c t_1 - _E t_1$	2·5 6·4	2·0 9·2	2·0 7·8	1·75 3·3	1·5 4·8	1·5 2·0	1·5 2·7	1·5 28·2	1·25 2·8
E $_c t_1 - _E t_1$	1·25 4·2	1·0 2·1	1·0 6·2	0·75 2·9	0·5 2·0	0·5 8·2	0·5 2·2	0·25 0·4	0·25 0·6

Frequency distribution for all rats

E Number	0 29	1 19	2 14	3 4	4 4	5 1	6 1

Mean = 1·2

Behaviour in the non-standard trials

There was little variation in the average off time per reward in the series of trials when the weight was increased daily, the size of reward remaining the same. But average active time per reward varied in the same manner as with the sprung bar (1956*a*). The values of $_R t_a$ and $_R t_o$ for the group of six rats given ten rewards at each weight are shown in table V. $_R t_a$ was small on the well practised 5 gm. trial, it increased when the weight was made greater, and then decreased during practice at 50 gm. The largest value of $_R t_a$ occurred on the 20 gm. trial for one rat, on the 30 gm. trial for one rat, on the 40 gm. trial for two rats, and on the 50 gm. trial for two rats.

TABLE V

ACTIVE TIME AND OFF TIME IN UNPRACTISED
BEHAVIOUR. VALUES FOR A GROUP OF SIX
RATS—10 REWARDS PER TRIAL

Day	gm.	$_R t_a$	$_R t_o$
7	5	0·55	8·4
8	5	0·55	9·4
9	10	1·5	9·7
10	20	2·1	8·1
11	30	2·2	8·6
12	40	2·4	7·0
13	50	2·4	8·7
14–18	Practice	at 50 gm.	
19	50	1·3	8·3

The performance of the rats given 20 rewards at each weight was similar to that described above. Some of the rats given only five rewards at each weight learned as well as those given more practice, but two of them were extinguished in the first

50 gm. trial, and had to be retrained. It may be significant (a question the writer is investigating) that these two showed the most pronounced repeating.

TABLE VI
PERFORMANCE WITH VARYING AMOUNTS OF REWARD

$\frac{1}{8}$ in. spaghetti		$\frac{1}{4}$ in. spaghetti		$\frac{1}{2}$ in. spaghetti	
$_R\bar{l}_a$	$_R\bar{l}_o$	$_R\bar{l}_a$	$_R\bar{l}_o$	$_R\bar{l}_a$	$_R\bar{l}_o$
1·5	7·4	1·4	7·8	0·9	14·0
1·4	5·5	1·2	10·4	1·3	21·1
1·1	5·6	1·4	7·5	1·4	14·5
1·0	8·1	1·6	8·6	0·8	13·0
1·1	5·4	1·4	15·4	1·5	21·8
0·9	6·1	1·3	17·6	0·8	12·2
Means 1·2	6·3	1·4	11·2	1·1	15·6

Table VI shows the values of $_R\bar{l}_a$ and $_R\bar{l}_o$ for groups of six rats given $\frac{1}{8}$ in., $\frac{1}{4}$ in. and $\frac{1}{2}$ in. pieces of spaghetti for each reward. Since the weight was the usual 50 gm. active time was not changed, but off time, being largely eating time (*vide supra*), varied with the amount of food.

Conversely, with a uniform $\frac{1}{4}$ in. of spaghetti for each reward, there was little variation of off times when the weight was changed. But $_R\bar{l}_a$ was reduced when the weight had the unusual value of 20 gm. and was increased for the unusual value of 70 gm. Reversion to 50 gm. from a smaller weight produced a greater active time, and reversion from a greater weight produced a smaller active time. The figures are in table VII.

TABLE VII
PERFORMANCE WITH UNUSUAL WEIGHTS ON THE BAR

Control 50 gm.		20 gm.		50 gm.		70 gm.		50 gm.	
$_R\bar{l}_a$	$_R\bar{l}_o$	$_R\bar{l}_a$	$_R\bar{l}_o$	$_R\bar{l}_a$	$_R\bar{l}_o$	$_R\bar{l}_a$	$_R\bar{l}_o$	$_R\bar{l}_a$	$_R\bar{l}_o$
0·9	7·1	0·9	6·2	1·2	6·9	1·2	7·4	1·5	7·8
1·1	11·4	0·9	10·5	1·3	8·5	2·0	10·0	0·8	5·5
1·5	7·9	0·6	7·8	1·8	7·2	2·1	8·0	0·7	6·2
Means 1·2	9·2	0·8	8·0	1·4	7·6	1·7	8·5	1·1	6·5

TABLE VIII
PERFORMANCE WITH VARYING FOOD DEPRIVATION

Hours	$_R\bar{l}_a$	$_R\bar{l}_o$	$_R\bar{l}_a$	$_R\bar{l}_o$	$_R\bar{l}_a$	$_R\bar{l}_o$	Means $_R\bar{l}_a$	$_R\bar{l}_o$
0	0·7	9·8	1·2	12·2	0·7	7·7	0·9	9·9
0	1·7	20·0	1·4	7·7	1·7	7·2	1·6	11·7
5	1·4	12·0	1·3	5·8	1·2	8·8	1·3	8·9
11	1·3	10·6	0·6	6·4	1·0	14·2	1·0	10·1
23	1·3	13·6	0·6	9·3	0·8	8·7	0·9	10·8
47	1·1	7·2	0·8	8·2	0·7	8·2	0·9	7·9

The values of $_R\bar{l}_a$ and $_R\bar{l}_o$ for six groups of three rats, with different periods of food deprivation, are shown in table VIII. There was no consistent variation of performance. The spaghetti used in the experiment, however, was not the food given in the home cages. The results suggest a special appetite for spaghetti, unaffected by satiation with ordinary food.

Behaviour during extinction

The conditions during the extinction trial were standard except that a contact produced the sound of the spaghetti gun, but no food. Table IX gives the values of $_E\bar{l}_a$, $_E\bar{l}_o$, $_S\bar{l}_a$ and $_S\bar{l}_o$, and the sounds-per-engagement ratio S/E, (a) for the whole 10-minute trial, (b) for the first ten engagements, and (c) for the last ten engagements.

TABLE IX

PERFORMANCE DURING EXTINCTION—NINE RATS

	$_E\bar{l}_a$	$_E\bar{l}_o$	$_S\bar{l}_a$	$_S\bar{l}_o$	S/E	Range of S/E
Overall	2·5	8·1	2·0	6·6	1·23	(0·56–1·8)
First 10 engagements	1·3	2·4	1·3	2·4	1·0	(0·5 –1·4)
Last 10 engagements	4·4	21·7	3·1	15·5	1·4	(0·2 –2·7)

Engagements started off as usual, since there was no indication that food would not be forthcoming, and intervals were short, since no time was spent in eating. As time went on both engagements and intervals became longer. Some rats, however, made long engagements, but did not keep on moving the knob up and down between the stops. Thus they made few or no contacts per engagement. Other rats developed repeating behaviour. The difference is shown by the large range of values of S/E in the last ten engagements.

The variety of behaviour occurring after about 10 minutes' extinction is illustrated by the longest engagements and the numbers of sounds they produced:—25·3 seconds, 0 sounds—23, 5—23, 1—22·3, 1—21·4, 13—16·7, 0—12·5, 2—10·9, 4—10·2, 4. These figures show beyond question that the number of contacts, or of events produced by contacts gives no reliable indication of the amount of behaviour.

DISCUSSION

There has been a great deal of research on bar-pressing behaviour, the results of which have been used in learning theory—see Skinner (1938), Hull (1943) and Munn (1951). Almost universally behaviour is described and (ostensibly) measured in terms of responses. The question naturally arises, "What is a bar-pressing response?"

A basic distinction has been made above between engagements as constituting the output, and contacts, etc., as part of the result. It is not always stated in the literature which, if any, of these is considered a response. Skinner (1938), the originator of bar-pressing research, gives an account indicating that he deals with contacts. With a simple weighted bar measurement of engagements is not possible. There have been modifications of the simple weighted bar and some different classes of responses, referred to in (1956a) but it seems likely that, in the majority of cases, a response is empirically a contact. What it is conceptually depends on the investigator's theoretical outlook, and whether its empirical basis gives warrant for its conceptual content is a matter of some doubt. As a basis for clear discussion the writer gives empirical translations of some commonly used variables.

Number of responses means empirically number of contacts (C). Rate of responses means rate of contacts, the reciprocal of the average total time per contact $(_c\bar{t}_a + _c\bar{t}_o)$. Latency of response means the time to the first contact $(_c\bar{t}_1)$ not $_E\bar{t}_1$. It can be seen, both from the principles stated above and from the experimental results, that by the limitation of discussion and experiment to contacts, a great deal of behaviour is over-looked. The use of the traditional term "response," however, tends to hide the paucity of data. In the writer's view, some of the reasons for this limitation are the use of the simple weighted bar, the lack of a sound rationale of bar-pressing research, and the practice of not watching the rat during an experiment.

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AFTER-IMAGES FROM RETINA AND BRAIN

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The evidence pointing to the retinal origin of after-images is considered. The reports of the occurrence of after-images from visual images of hallucinatory vividness are reviewed.

Experimental results are presented to indicate that a complementarily coloured after-image may arise following the exposure of the temporarily blind retina to a coloured stimulus.

After-images, or after-effects, from vivid images are described in seventeen persons (mostly possessors of "number-forms"). They are found to move with the eyes and to show, in some persons, a degree of conformity with Emmert's Law which, while considerable, is less than that of after-images of real stimuli. In the case of one "eidetic" subject, the after-images from neither real nor imaged stimuli conformed with Emmert's Law. In some persons, after-images of images occur in complementary colours.

The retinal origin of after-images is affirmed, but that they can occur occasionally as a purely central phenomenon is acknowledged. The possible learned or inherent nature of after-images of central origin is discussed.

I

INTRODUCTION

The problem of the retinal or central origin of visual after-images was for many years the subject of intense controversy. That after-images can be solely of peripheral origin has been shown by the fact that an after-image (hereinafter referred to as AI) may follow the exposure of a light-source to an eye which is temporarily blind, the blindness being caused by local pressure on the eyeball. The clearest demonstration of this is that of Cibis and Nothdurft (1948). The method had previously been applied to the same problem by Exner (1879) and Craik (1940). Cibis and Nothdurft covered one eye and made the other blind. The blind eye only was then exposed to an electric-light bulb, and later an AI was seen when the pressure was released. Their experiment did not relate to coloured light. Exner's (1879) experiments had included a coloured source and he failed to obtain AIs. Continental writers have emphasized the role of the cerebrum in colour-vision, describing cases of achromatic vision consequent on brain injuries. Among observations which are to be described below are some in which complementarily coloured AIs occur as central phenomena. It is therefore pertinent to consider the use of pressure-blindness to determine whether complementarily coloured AIs can have a retinal origin.

One source of evidence relating to AIs of purely central origin derives from the occurrence of AIs from hallucinated stimuli. This is a possibility that seems often to have been regarded as not quite respectable.

Gruithuisen (1812, p. 256) described how, on awakening from sleep, he experienced AIs from the visual phenomena of dreams. He experienced negative AIs ("umgekehrten Tauschungen") over a hundred times. He once had a positive AI. Dreaming of a lightning flash, he awoke, and observed an AI of "feeble light" which gradually faded to become darker than its surroundings. He also described how he dreamed of violet fluorspar on a fire of glowing coals, and on awakening, had an AI which moved with his direction of regard, and appeared as a yellow spot on a darker area, like the

sun in a stagnant pool (". . . die Gestalt der Sonne in einem stehenden Wasser hatte").

Alexander (1904) was to describe similar AIs of dreams. He mentions an example of a dream image of a human figure, "and when my eyes suddenly opened, I was surprised to see the figure lengthen out exactly as does an after-image."

Wundt (1863, p. 387) wrote that if a brightly-coloured image is regarded for a long time with eyes closed, on opening the eyes, and viewing a plain white surface, the image may persist briefly in its complementary colours ("so sieht man auf dieser kurze zeit das Phantasiebild in den Ergänzungsfarben fortbestehen"). The statement does not appear in the second edition, and may be among those "wild oats of my youthful days, which I would gladly have forgotten" (preface to the second edition, 1892).

The Salpêtrière workers, Binet, Féré and Charcot, were familiar with Wundt's view as quoted by Ribot (1882, p. 20), and with the fluorspar episode of Gruithuisen, as given by Burdach (1839, p. 207), the AI being described as a yellow spot "sur un fond bleu." Féré (1885) found he was able himself to obtain a green AI from a red imaged figure, but "très peu de sujets sont capables de la répéter."

It was found (Binet and Féré, 1898) that if they caused a coloured hallucination to be perceived by their hypnotic subjects, there followed an AI in the complementary colour.

Franz (1899) remained sceptical of the naïvety of the subjects despite the remark of Binet and Féré (1898, p. 253) that "it would be unreasonable to maintain that an hysterical woman, who scarcely knows how to read or write, knows the theory of complementary colours. Our subjects have always answered correctly and the correct answer has been given when the experiment was performed for the first time."

In St. Petersburg the same phenomenon was independently described by Sreznewsky and Bechterev (Bechterev, 1906) using hypnotized subjects.

William James (1890, p. 67) quoted the case of Meyer, who conjured up a vivid image of a stirrup with his eyes closed and, upon opening the eyes, "saw its after-image." Alexander (1904) pointed out that Meyer, in the passage quoted, does not state that he saw negative AIs as James assumes, and that there was no reason why the example given was not a projection of the original image. Alexander himself could still see images of chessmen with his eyes open an hour after playing chess, and their size was proportional to his distance from the background on which they were projected. He wrote, "some mental images follow precisely the law of after-images." He therefore disputed James' "universal proposition, that after-images seem larger if we project them on a distant screen, and smaller if we project them on a near one, whilst no such change takes place in mental pictures" (James, 1890, p. 51). He implicitly challenged Fechner's principle that such mental pictures do not move with the eyes.

Franz's remarks, in regard to the observation of Binet and Féré, led Downey (1901) to seek a totally naïve subject, and she gives an excellent account of a series of observations in which an image of a coloured form was followed consistently by a brief AI in the complementary colour. If Downey suggested to the subject that she should try and see a colour other than the complementary, there was delay while the subject first suppressed the spontaneous, complementary AI, and then imaged the suggested colour.

In studies of eidetic imagery, Jaensch (1930) found that some children could imagine a coloured object, then look at a screen and see the AI in a complementary colour. Klüver (1928) stated he had found the same phenomenon in eidetic subjects. Jaensch regarded it as a manifestation of extreme eidetic ability.

Dorcus (1937) carried out experiments on AIs of suggested colours with hypnotized subjects, but failed to obtain the older hypnotists' results. His hypnotic technique is not described and, in accord with the fashion of the day, he does not state if he obtained introspections from his subjects as to whether they had even experienced hallucinations. Since visual hallucinations are not the easiest hypnotic phenomena to obtain, this obviously bears on his results.

Erickson and Erickson (1938), using naïve subjects, confirmed the observations of the earlier hypnotists, that complementarily coloured AIs may follow hallucinated colours. They showed the same extraordinary patience and care in ensuring a profound trance that has marked much of Erickson's reported hypnotic work.

Hibler (1940) failed to confirm the Ericksons' results, but gives the impression that such was his intention when he embarked on his experiments. He did not use naïve subjects or a comparable technique (Erickson, 1941; Hibler, 1941), but obtained similar results. These results he rejected because when the subjects were rapidly awoken from the trance state the complementary AI no longer persisted. However, Downey, Weiskrantz (1950) and the writer's results reported below are in agreement that the spontaneous AIs from imaged stimuli generally last only about 5 seconds. In the hypnotic state it might be argued that the AI would receive its hue spontaneously, but because the subjects believed there to exist a genuine coloured piece of paper, would persist as an active haullucination.

Weiskrantz (1950) related the size of AIs from an imaged stimulus to Emmert's Law. He described the case of a woman (who was not naïve) who obtained negative AIs from an imaged black square. No chromatic AIs could be obtained. The sizes of the AIs corresponded closely with those produced by a real stimulus and those predicted by Emmert's Law.

Emmert's Law states that the linear size of an AI varies approximately linearly with the distance of the surface on which it is projected from the observer. $\dfrac{D_1}{D_2} = \dfrac{S}{I}$

(S and I being the linear size of stimulus and AI respectively; D_1 and D_2 being distance from observer to stimulus, and observer to surface on which the AI is seen, respectively),

or $\dfrac{D_1}{D_2} \cdot \dfrac{I}{S} = K_E$. Calculation of K_E provides a convenient means of determining the degree of conformity of an image size with Emmert's Law (for complete conformity K_E being equal to unity). This function K_E is therefore used in all the tables of measurements recorded below.

II

EXPERIMENTAL METHOD AND RESULTS

(a) It was confirmed that, using one eye only, an AI may follow exposure of an electric-light bulb to the pressure-blind eye. The method was then applied to a coloured stimulus. A rectangular strip of red paper about 6 inches by 12 inches, of good saturation, was placed by an assistant about 2 feet in front of the already blinded eye of the writer in a previously agreed position on a plain white wall. "Fixation" of the position was then maintained for 20 seconds. The red paper was removed and the pressure released. With return of vision the wall was viewed for an AI. It had previously been determined that fixation of the paper for 20 seconds with a normal eye would give a bright green AI for 12 to 15 seconds, with the aid of blinking.

Peripheral, coloured AIs are not easily seen, and it was not till the ninth attempt that an unequivocal, bright green, rectangular and centrally-placed AI, which could be seen for about 15 seconds, was obtained. Evidently the correct spot had finally been fixated. After six more attempts another green AI was obtained.

(b) The subjects described below were mostly drawn from a larger group known to possess spontaneous vivid imagery. Fifteen of the total of 17 subjects possessed "number-forms" or analogous visual imagery for months, days, alphabet or family, of the type first described and discussed by Galton and later by many others, including Pear (1922). The subjects, with one exception, were Royal Air Force personnel, and were all naïve at least in regard to Emmert's Law and the rules of complementary colours of AIs.

The first question put to each subject was "Can you close your eyes and see a red cross?" (or a black square). If the response was affirmative—"Keep looking at it for half a minute. I shall time you. When I ask you to open your eyes, I want you to do so and just look at the wall in front of you." The subject had been previously positioned in front of a plain wall (generally of a light buff colour). When the subject had opened his eyes, he was asked, "Did you see anything on the wall?" If the reply was affirmative, efforts were made to investigate the matter further, but this was not always possible. Many of the subjects were available only for an hour or two. All observations made on such subjects are recorded below without selection. Twenty possessors of number, or other, "forms" were questioned and fifteen found to experience after-effects of images.

On many of the subjects a series of measurements of the sizes of AIs were undertaken, using a method similar to that of Weiskrantz (1950). The subject fixedly regarded a faint pencil dot and imaged a square, the corners of which were given by four very small, faint dots on a large sheet of off-white paper, there being a light and fairly uniform background. When a square could be perceived, fixation of it was maintained for half a minute, after which the subject looked at a central spot on a very large sheet of off-white graph paper, the illumination of which was only very slightly greater than that of the stimulus paper. His instructions guided the experimenter's pointers on the graph paper, to obtain the size of the AI. Control experiments showed that the faint dots on the stimulus paper were incapable of producing AIs. The dots formed a 2-inch square in the majority of cases, but occasionally other sizes were used. The size was not known by the subject. Similar observations with real stimuli were also carried out. The distances from the subjects' eyelids to the stimulus, and from the subjects' eyelids to the screen, were measured in every observation by tape measure.

Subject Ho.

A twenty-six-year-old medical officer, a colleague of the writer's, working as a plastic surgeon. He imaged a red cross with eyes closed and saw a large, dark green cross on eye-opening. This was repeated. A violet square became a yellow square on eye-opening. The imaged violet square had had a yellow edge around it. An image of an orange became a yellow-green on eye-opening and flashed to sky-blue when he blinked. A green square became red. Blue became a lime-green.

The subject remarked, "If I look at a light colour, when I open my eyes it reverses to dark and vice versa. I seem to see green easily. Is it because when you look away from a bright electric light you see a green horseshoe?"

At this point, and subsequent to the above observations, he was questioned in regard to his preconceived ideas, if any. It may be pointed out that a knowledge of the phenomena of AIs is not required of, nor possessed by, the majority of medical students or practitioners. He proved remarkably naïve and denied any knowledge of the rules of complementary colours. His only suggestion of mechanism was to repeat his remark about the electric-light bulb.

He was asked to try and see a colour other than the complementary when he opened his eyes, but reported, "When I'm concentrating on a coloured object I can't think of anything else or the object goes. I'm too busy concentrating on it to think of anything else, and when I do open my eyes it's too quick—there's no time to think."

Having had the "practice" of evoking images he found (as did others) it became easy to evoke them with eyes open. He was quite intrigued with the phenomenon, and pursued it silently on his own. He reported that an image of an orange had a halo of green around it. When he stopped trying to see the orange he suddenly saw a green image which abruptly shrank. A red cross with eyes closed became green on eye-opening, merging into a green circle, the peripheral corners seeming to expand, the central corners to fill in. The primary images and the AIs moved with eye-movements.

Observations with Subject Ho, which were subjected to measurement, are shown in Table I. In this and the other tables the size of the AI, as predicted from Emmert's Law, is given for each observation, together with the function K_E.

TABLE I
(Subject Ho.)

Obsn.	D_1 (ins.)	D_2 (ins.)	Stimulus			AI		Emmert (ins.)	K_E
			Imaged	Real	Size (ins.)	(ins.)	Colour		
(a)	41	70	Black		2	6	Black	3·5	1·7
(b)	41	70	Light Green		2	8	Dark Green	3·5	2·3
(c)	24	68	Black		2	11	Bright	6	1·8
(d)	45	87	Dark Green		2	9	Yellow	4	2·2
(e)	32	90	Grey		2	17	Bright	6	2·8
(f)	32	90		Blue	2	7·5	Yellow	6	1·3
(g)	48	90		Grey	2	4	Bright	4	1·0

After observation (d) the subject remarked that it seemed that the size of the AI was proportional to distance, and he proposed the appropriate equation, of which he had no previous knowledge. It is nevertheless apparent that the AIs to imaged stimuli, while showing a general trend towards increase of size with increase of $\frac{D_2}{D_1}$, did not conform to Emmert's Law, unlike real stimuli.

This subject's AIs to imaged stimuli lasted up to 10 seconds.

TABLE II
(Subject Ch.)

Obsn.	D_1 (ins.)	D_2 (ins.)	Stimulus			AI		Emmert (ins.)	K_E
			Imaged	Real	Size (ins.)	(ins.)	Colour		
(a)	19	60	Grey		2	5	Grey	6·0	0·8
(b)	19	78	Grey		2	7	Grey	8·0	0·9
(c)	25	52	Grey		2	4	Grey	4·0	1·0
(d)	15	52	Grey		2	5	Grey	6·9	0·7
(e)	15	53		Grey	2	6	Bright	6·9	0·9
(f)	23	42	Grey		2	3·5	Grey	3·5	1·0
(g)	23	107	Grey		2	7·5	Grey	9·3	0·8
(h)	23	107	Dark Grey		2	9	Bright	9·3	1·0
(i)	23	78	Grey		2	nil	—	—	
(j)	23	78	Grey		2	6	Grey	6·8	0·9
(k)	23	78		Grey	2	7	Bright	6·8	1·0
(l)	42	76	Grey		2	4·75	Grey	3·6	1·3
(m)	42	76		Grey	2	3	Bright	3·6	0·8
(n)	51	93	Grey		2	3·5	Grey	3·6	1·0
(o)	51	93		Grey	2	3·75	Bright	3·6	1·0
(p)	70	30	Grey		2	3	Grey	0·9	3·5
(q)	70	30		Grey	2	1·4	Bright	0·9	1·6
(r)	70	30	Grey		2	2·5	Grey	0·9	2·8

Subject Ch.

A building apprentice aged nineteen, National Service, grammar school education, chief interest handicrafts, negligible physics. Totally naïve in regard to AIs. At the end of the series of observations the size-distance relation was explained to him. He denied having realized the relation and seemed to have some difficulty in grasping it.

He could not image colour, but could image a grey square, which moved with shifting of gaze. On glancing away to the screen, the square persisted only for a couple of seconds, in a new size and generally as a grey square. The indication of size was obtained from what might be called the primary memory image of the square—no actual perception of a square being present at the time the pointers were finally positioned. This should be borne in mind in considering the results in Table II, which show a remarkable conformity to Emmert's Law. It will be noted that the relation breaks down, for imaged stimuli, when the ratio of D_1 to D_2 is greater than unity. After observation (*q*) with a real stimulus the subject remarked that the AI was much smaller than the previous one.

This subject cannot, except in observation (*h*), be said to have experienced AIs from imaged stimuli, but rather the persistence of the original image.

Subject Ha.

A nineteen-year-old airman of secondary modern school education. Questioning after the observations revealed him to be completely naïve in regard to the normal properties of AIs. He possessed a circular "month-form," an "alphabet-form" with a red "H," and the idea of Sunday or Tuesday was accompanied by a sensation of white or blue respectively. This varient of synaesthesia is quite common.

Questioning elicited:—(*a*) with eyes closed a black cross changing to a large white cross on looking at the wall, (*b*) a red cross changing to a large dark cross and shrinking to a ball on eye-opening, (*c*) a black cross which became a large black cross on eye-opening. The subject remarked spontaneously that there was a "brightness" round the edges of the black figure. (*d*) Again, later, a black cross, but this he perceived as a crucifix type with himself regarding it obliquely, looking from an angle along its long axis. The AI was black, but the "horizontal" arm of the cross was now the longest, while previously it had been subjectively the shorter arm.

TABLE III
(SUBJECT HA.)

Obsn.	D_1 (ins.)	D_2 (ins.)	Stimulus Imaged	Size (ins.)	AI (ins.)	Colour	Emmert (ins.)	K_E
(*a*)	125	40	Blue	6	2	Yellow	1·9	1·0
(*b*)	30	59	Black	2	4	Bright	4·0	1·0
(*c*)	16	34	Green	2	3 tilted	Green	4·0 + tilted	0·7
(*d*)	133	39	Blue	6	2	Light Green	1·8	1·1
(*e*)	41	118	Blue	2	5	Blue	5·8	0·9
(*f*)	22	64	White	2	3	Dark	6·0	0·5
(*g*)	22	64	Black	2	7·5 tilted	Black	8·0 tilted	1·0

Further observations with this subject are shown in Table III. Unfortunately, more were not possible as, at the time he was seen, he was going overseas.

Observations (*c*) and (*g*) were of especial interest. On glancing at the screen his square had tilted, slightly in (*c*) and through 45 degrees in (*g*).

Subject Wi

A senior N.C.O., aged 37, a trained mental nurse of average intelligence. A red cross with eyes closed became, on eye-opening, a large orange cross which passed through orange to green, the green becoming circular and shrinking.

It may be pointed out that the knowledge of psychology possessed by mental nurses does not normally extend to AIs. At the end of the observations described in Table IV, the subject was questioned and denied having any idea of the purpose of the observations, or having any knowledge of AIs, let alone Emmert's Law.

TABLE IV
(Subject Wi.)

Obsn.	D_1 (ins.)	D_2 (ins.)	2-in. Stimulus		AI (ins.)	Emmert (ins)	K_E	Remarks.
			Imaged	Real				
(a)	26	56	Black		5	4·3	1·2	Green, rapidly to white, slowly shrinking to small bright spot. 20 secs.
(b)	26	56	Yellow		4·75	4·3	1·1	Grey, shrinking—becoming bright blob. 20 secs.
(c)	26	56	Blue		4·5	4·3	1·0	Light blue to green, to yellow, to white spot. 20 secs.
(d)	26	56		Blue	8	4·3	1·9	Yellow, to 4 in. square, shrinking to small white circle. 30 secs.
(e)	26	56		Blue	6	4·3	1·4	Yellow, to 4 in. square, to white spot. 30 secs.
(f)	16	43	Black		4·25	5·4	0·8	White, shrinking. 20 secs.
(g)	16	43		Grey	4·75	5·4	0·9	White, shrinking. 20 secs.
(h)	24	82	Red		5·75	6·8	0·8	Cream, became white circle, shrinking. 45 secs.
(i)	24	82		Grey	6·5	6·8	1·0	White, became grey, disappeared expanding outwards. 15 secs.
(j)	23	70	Black		6	6·1	1·0	Light grey, became circular, shrinking. 30 secs.
(k)	23	70		Grey	6·25	6·1	1·0	White, shrinking. 30 secs.
(l)	35	67	Apple Green		4	3·8	1·0	Light blue, shrinking through white, circular. 20 secs.
(m)	35	67		Grey	4	3·8	1·0	White, shrinking, becoming circular. 20 secs.

This subject's AIs from imaged stimuli lasted much longer than other subjects', generally persisting 20–30 seconds. During this time they became circular, underwent shrinkage, colour changes, and often a curious sense of pulsation of size, as if getting slightly bigger and smaller. Investigation of the pulsation rate was subsequently carried out and will be described elsewhere. The rate was that of his arterial pulse. His AIs to real stimuli also showed pulsation, but this was much less marked.

Subject Gr.

A National Service Corporal, aged 22, a trained mental nurse. Subsidiary Higher School Certificate in Physics. Discussion, after the observations described below, revealed he previously realized the existence of AIs and was vaguely aware of their colour relations, though not of size relations. There was no reason to doubt his trustworthiness. He stated that he was at first "amazed" to see any AI as a result of imaging objects. By the end of the observations he realized the size changes might obey the rules governing cinema projection, but thought his AIs much too large.

He imaged a red cross with eyes closed. On eye-opening he saw a much bigger green "blob" which genuinely surprised and interested him. A mauve cross was succeeded by a large, yellow, vertical streak, lasting 15 seconds. A green rectangle was succeeded by a large red one. A yellow rectangle became grey. These AIs all moved with change of direction of gaze.

His detailed results are given in Table V. Observations (a) to (e) were made on one day and show a remarkable conformity with Emmert's Law. The remaining observations

were made a month later. After observation (p) the subject remarked, "That seems too big, doesn't it?" The AI in (r) underwent growth upwards by about an inch after the first second or so. His AIs from imaged stimuli lasted 5–10 seconds.

TABLE V
(Subject Gr.)

Obsn.	D_1 (ins.)	D_2 (ins.)	Stimulus			AI		Emmert (ins.)	K_E
			Imaged	Real	Size (ins.)	(ins.)	Colour		
(a)	15	49	Black		2	5	White	6·5	0·8
(b)	15	81	Black		2	11	White	11	1·0
(c)	15	59	Black		2	8	White	8	1·0
(d)	14	32	White		2	4·5	Dark	4·6	1·0
(e)	140	42	Black		6	1·75	White	1·7	1·0
(f)	80	100	Weak Orange		6·6	8·5	Grey	8·2	1·0
(g)	80	100	Blue		2	5·5	Pink	2·5	2·2
(h)	80	100		Grey	2	3	White	2·5	1·2
(i)	50	82	Red		2	nil	nil	—	—
(j)	50	82	Black		2	4	White	3·3	1·2
(k)	50	82	Black		2	4·5	White	3·3	1·4
(l)	50	82		Grey	2	3	White	3·3	0·9
(m)	63	28	Black		2	1·2	Pink	0·9	1·3
(n)	95	34	Black		2	1·4	Yellow White	0·7	2·0
(o)	95	34		Grey	2	0·8	Yellow	0·7	1·1
(p)	34	108	Green		2	7	Bright	6·4	1·1
(q)	22	110	Dark Purple		2	12	White	10	1·2
(r)	77	105	White		2	4	Grey	2·7	1·5
(s)	77	105	Black		2	7	Bright Pink	2·7	2·6
(t)	77	105		Black	2	3	Yellow	2·7	1·1

Subject Wh.

A Sergeant, a trained mental nurse, aged 29. He claimed to be naïve in regard to the whole subject. He possessed visual imagery of hallucinatory vividness, which conformed with those criteria which were formerly considered to indicate "eidetic" ability. He could regard an object briefly, then turn away and see it wherever he wished, the image being "literally seen" in its original colours, though he was aware of it not being real. He experienced colour sensations with emotions.

It may be noted that the AIs of real stimuli, perceived by eidetic subjects, have been noted elsewhere as deviating markedly from Emmert's Law (Allport, 1924; Meenes and Morton, 1936).

Initial questioning elicited a clear red cross with eyes closed, which became a huge red cross on eye-opening. His detailed results are given in Table VI. His AIs from imaged stimuli lasted up to 3 or 4 seconds and moved with the eyes. He was asked, as previous subjects had been, to indicate the height of the AI. He complied with the request and until observation (k) it never occurred to him to remark on the width. In observation (k), fixation of a real blue 2-inch square was followed by a yellow AI, which he sketched for me, extending 4 inches below and 10 inches above the fixation point. Questioned, he stated calmly that it had behaved in the same fashion as all the previous AIs, the upper half giving the impression of elongating upwards. Asked why he had not mentioned this before, he replied that he had not been asked for anything except the height, and that he had assumed the elongation to be probably part of the natural response being studied. How great a part the experimenter's interest in the height had influenced the AIs up to this point, can only be surmized. Certainly they show a remarkably constant tendency to be about twice the expected height.

The colour of the AI in observation (*a*) was not recorded immediately, attention being directed to size; it was considered dark in retrospect. After the green AI in (*b*) the subject spontaneously expressed himself surprised, and asked why it should ·be green, when he

TABLE VI

(Subject Wh.)

Obsn.	D_1 (ins.)	D_2 (ins.)	Stimulus			AI				K_E
			Imaged	Real	Size (ins.)	Colour	Height (ins.)	Width (ins.)	Emmert (ins.)	
(*a*)	23	60	Red		2	Dark	11	—	5·2	2·1
(*b*)	23	60	Red		2	Green	10·5	—	5·2	2·0
(*c*)	23	60	Orange		2	Light Green	10·5	—	5·2	2·0 .
(*d*)	20	40	Black		2	Black	6·5	—	4	1·6
(*e*)	20	40	Yellow		2	Green	6	—	4	1·5
(*f*)	22	81	Green		2	Green	16·5	—	7·4	2·2
(*g*)	22	81	Violet		2	Violet	14	—	7·4	1·9
(*h*)	128	40	Black		6	Black	4	—	1·9	2·1
(*i*)	128	40	Black		6	Black	3·5	—	1·9	1·9
(*j*)	27	78	Green		2	Dark	9	—	5·8	1·6
(*k*)	27	78		Blue	2	Yellow	14	7	5·8	2·4 or 1·2
(*l*)	27	78		Blue	2	Yellow	14	7	5·8	2·4 or 1·2
(*m*)	27	78		Blue	2	Yellow	14	7	5·8	2·4 or 1·2
(*n*)	132	42	Black		6	Green	8		1·9	4·2
(*o*)	28	107	Black		2	Green	18	9	7·6	2·4 or 1·2
(*p*)	19	56	Green		2	Dark	12	6	5·9	2·0 or 1·0
(*q*)	19	40	Yellow		2	Red	7	3·75	4·2	1·7 or 0·9
(*r*)	19	40	Green		2	Dark	5	5	4·2	1·2
(*s*)	19	40		Grey	2	Green	6	6	4·2	1·4
(*t*)	31	120		Green	4	Green	11	—	15·5	0·7

had been seeing red. It will be noted that many of this subject's AIs are not complementarily coloured; green predominates. Observations (*a*) to (*i*) were made at the same session, (*j*) to (*n*) a week later, (*n*) being made some time after (*m*), the interval being occupied by considerations of the subject's edetic images of real objects. These eidetic images were usually slightly larger than the real objects and showed no size-distance relation. It is possible that the size of the AI in (*n*) was influenced by the "set" derived from the eidetic images. Observations (*o*) to (*t*) were made a further week later. The subject remarked that the AI of observation (*r*) was the first one he had ever experienced as a square.

This subject's imagery was exceedingly labile and probably the AIs were greatly influenced by his attitude. Nevertheless, they appeared spontaneously and immediately on glancing at the screen, whereas the stimulus image, which was viewed for half a minute, always involved effort and delay before appearing.

Subject Ga.

An intelligent, nineteen-year-old airman, grammar school education (Arts subjects). Enquiry had previously elicited that he experienced different colour sensations when he thought of different days of the week. If he thought of Tuesday, for instance, with his eyes closed, he experienced a vivid sensation of green; with eyes open it was an "impression" only.

He was asked to visualize a red circle with his eyes closed and stare at it for a full minute; on eye-opening he saw a green circle on a grey screen in front of him. Asked to visualize blue, he said he saw a turquoise; on eye-opening this became a "sort of light green, not like the other green, this is a light colour." When asked to see a red circle and to try not to go on seeing it on eye-opening he was able to do so. A green oval, visualized for 45 seconds with eyes closed, became a pink oval on the cream-coloured wall. Navy blue was succeeded by a "light colour." The subject remarked at this stage, that it seemed he was experiencing the complementary colour. He denied, on questioning, any previous knowledge of AIs, or that he had had any preconceived ideas as to what he would experience on opening his eyes.

He was asked to try to see yellow after visualizing red. He found, as did other subjects (and Downey's case) that he first had to suppress the spontaneous, green AI which appeared without effort. He could then perceive a yellow which grew more intense with time, whereas the spontaneous, effortless AI was at its most vivid initially.

His AIs from images moved with the eyes and lasted about 5 seconds. An image of a light bulb became a large dark shape, when he opened his eyes and regarded a distant wall.

Subject Hd.

A twenty-one-year-old airman, with "coloured months" and a "week-form." Wholly naïve in regard to AIs. He was genuinely surprised to see a large green square when he opened his eyes after imaging a red square.

Subject Bo.

A twenty-three-year-old housewife, who experienced colour sensations when she thought of certain months of the year, certain personalities or certain days of the week. The eyes being closed, the idea of Monday or Wednesday or Friday was accompanied by a blue or red or green sensation respectively.

She was able to visualize a black square on a large sheet of white paper on which were very faint dots forming a square 1·1 inches in height. This she regarded for 45 seconds at 22 inches from her eyes. On looking at the blank wall 19 feet away, she experienced a grey square, which faded in about 10 seconds. This AI was measured and found to be 12 inches in height. By Emmert's Law the predicted height of the AI would be 11·4 inches. Unfortunately time did not allow further observations.

Subject Od.

A twenty-year-old airman. No "forms." Able with difficulty to visualize a red cross with eyes closed; saw a "mustard-coloured patch" on the buff wall on eye-opening.

Attempts to image a square with eyes open, on the stimulus paper used with other subjects, proved very difficult and he could do so only once. A 2-inch blue square became a "very vivid" blue square on the screen, tilted through 45 degrees (cp. Case *Ha*). This was of only 1·6 inches vertical height from corner to corner. $D_1 = 34$ inches, $D_2 = 55$ inches.

Subject N.

A twenty-four-year-old Corporal visualized a black cross with eyes closed. On eye-opening it was momentarily seen projected on the wall, turned through about 45 degrees.

Two other subjects repeatedly got large, bright, projected images from dark ones, on eye-opening, but could not image a square with eyes open. A pink square, with eyes closed, remained pink on eye-opening for one of these subjects.

Four other subjects were found, in whom the image of a black or coloured object briefly persisted unchanged on eye-opening (*cf.* Alexander's remarks on James' account of Meyer, and Subject Ch. above).

Weiskrantz (1950) used as a control a non-naïve subject familiar with Emmert's Law. His calculated sizes for AIs, when he sat in the experimental situation, deviated grossly from the sizes which should be predicted, and bore no comparison with the sizes of the genuine, spontaneous AIs from an imaged square, of Weiskrantz's case.

Similar control results were obtained by the writer. If, however, the control, a non-naïve subject, of higher educational level than most of the fifteen cases described above, was not required to fixate the imaginary square, but allowed to look where he liked, his estimates were considerably more accurate, though less than for the genuine AIs. His whole appearance was that of one making a shrewd guess—looking at the distance involved,

making slight movements of the head to judge the distance the better. A complete contrast to those who reported the presence of the image on fixation of the stimulus paper; they were concentrating with fixed gaze and, if they spoke, would often do so in a characteristic "careful not to distract" manner.

III

DISCUSSION

The conclusion may be drawn from the observations described that AIs can occur as a purely central phenomenon. Filehne (1885) described how disappearance of an existing AI could be effected by means of pressure-blindness. Cibis and Nothdurft (1948), using a black and white figure as stimulus, studied this phenomenon and noted that they could still see an AI during a brief period, when awareness of the environment had just been caused to disappear by pressure on the eye. This AI they believed to be of wholly central origin. Central AIs are very individual matters and the writer, in the course of many experiments, has never observed an AI under the conditions described by Cibis and Nothdurft, unless some vision was still present. The writer's criterion of complete blindness was unawareness of a *moving* source of bright light. This is a more total form of blindness than mere unawareness of the static scene. On this account, and in view of McDougall's (1901) work on the unfluence of attention in causing awareness and fading of different parts of a visual field, Cibis and Nothdurft's belief that they were definitely seeing central AIs in all cases, should perhaps be regarded with caution.

It is pertinent to recall the local reduction of sensitivity in the visual field of one eye, following stimulation of the corresponding area of the other eye (Dunlap, 1921, Crook, 1930). This represents a form of central adaptation such as may accompany central AIs.

Scrutiny of the accounts of AIs following images or hallucinations as reported by others, and those here recorded, confirm that spontaneous AIs in complementary colours may occur. This colour relation, while a general finding, has exceptions, particularly if the eyes are open when the primary image or hallucination is perceived.

Alexander's (1904) doubts of James' "universal proposition" are confirmed; mental pictures can undergo size changes when projected on a distant screen and, as Alexander pointed out, those size changes frequently show conformation with Emmert's Law. The degree of conformation varies with different individuals, and is generally less exact than is the case with AIs of real objects. The "eidetic" subject's AIs from real and from imaged stimuli, deviated markedly from the sizes predicted by Emmert's Law.

Alexander's challenge of Fechner's widely quoted distinction between memory images and AIs, that only the latter move with the eyes, is supported.

The conformity of mental images with Emmert's Law demonstrates a cerebral correlate of angular size of a part of the whole visual field. This cerebral process persists in the absence of visual stimulation. The tentative conclusion might be drawn that the construction of a vivid image may involve many of the same neurones as are involved in the perception of a similar real stimulus.

The occurrence of a complementarily coloured and negative AI from an imaged stimulus might be learned or inherent.

If the response was a learned one it could be considered as a conditioned response, the unconditioned responses having occurred in past experience. Experimental "sensory conditioning" has always been difficult to achieve. That it can occur, if distractions are excluded, was shown by Leuba (1941) in a straightforward conditioning situation. Leuba obtained "limitation of the spontaneous mental life of the subject

and the consequent limitation of attention to the stimuli provided by the experimenter" by using hypnosis.

Popov (1953), under the relaxed, quiet and darkened conditions of electro-encephalographic recording, demonstrated that AI's can occur as conditioned responses and that the conditioned AI of a white light may show the same colour changes as the unconditioned AI response (Popov and Popov, 1953).

Alternatively, the complementary AI from a coloured image might result from central "fatigue," and be inherent in central colour perception mechanisms. McDougall carried out a series of experiments on the central inhibition of visual stimuli and demonstrated "monocular struggle," in which, under certain conditions, if light of two different colours falls on the same area of the retina of one eye, perception of first the one and then the other colour may alternate (McDougall, 1901, Observation 27). This experiment is easily carried out and has been confirmed by the writer using McDougall's method. McDougall's experiments led him to postulate a central origin for the "flight of colours" in the AI of a white light, and that central fatigue of one primary colour "system" may occur, with the appearance of another and inhibition of the first. An analogous fatigue process of one colour "system," might play consequence, a relatively active, unfatigued, alternative colour "system," might play a part in giving rise to AIs from images.

It may in conclusion be re-affirmed that perception of an AI normally arises in the presence of corresponding stimulation of nerve endings in the retina, but that in some persons it may occasionally arise solely as the result of cerebral activity. It is further believed that the nervous system will, at all levels, retinal and central, "register" a change of the input applied to it, and that this situation normally follows stimulation by light.

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TWO THEOREMS IN COLOUR VISION

BY

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Exact definitions and statements are given of some widely accepted concepts and assumptions in colour vision. Two theorems are stated, and one of them is proved (the other being self-evident). Two applications of the theorems are briefly discussed.

INTRODUCTION

The two theorems which are stated here have wide application to the theory of colour vision, and have in fact been applied long ago; they are implicitly assumed in the deduction by von Kries (1878) from the trichromatic theory that colour matches should not be upset by adaptation. Neither, however, has been explicitly stated before, nor have the assumptions on which they are based been examined. The justification for stating them now is that certain hypotheses concerning mechanisms of colour vision which have recently been put forward can, if the two present theorems be accepted, be conclusively disproved. Two such applications of the theorems are briefly indicated at the end of the present paper, and one of them is treated in more detail elsewhere (Brindley, 1957).

DEFINITIONS

For a given region of retina in a given subject, colour matching is said to be *trichromatic* if any four lights, whether spectrally homogeneous or not, can be divided into two groups such that the sum of multiples of the members of one group forms a perfect match for the sum of multiples of the members of the other; and if this is not in general true for any three lights (modified from Pirenne, 1948).

Two receptors x and y are said to belong to the same *class* if whenever two lights A and B of different spectral composition are such that the response of x to A is the same as the response of x to B, then the response of y to A is the same as the response of y to B; and if the same relation holds when "x" is substituted for "y", and "y" for "x".

STATEMENT OF THE THEOREMS

Theorem 1

If a light A produces the same response in all the receptors of a given area of retina as a light B of different spectral composition, then the lights A and B match when viewed with this area of retina.

Theorem 2

If a region of retina for which colour matching is trichromatic contains only three classes of receptors, and two lights A and B of different spectral composition match when viewed with this area of retina, then light A produces the same response in all the receptors as light B.

Theorem 1 is self-evident, if it be accepted that a subject can obtain no information about a light except through the responses of his receptors. Theorem 2 requires proof.

Assumptions Required in the Proof of Theorem 2

Postulate of unidimensional response

For any lights A and B of different spectral composition, a number n can always be found, such that for a given receptor, nB provokes the same response as A.

Substitution postulate

If a receptor x gives the same response to a light A as to a light B, then A can be substituted for B as a constituent of any mixed light without altering the response of x to it: (A + C) always has the same effect as (B + C), where C is any light. Reciprocally, if (A + C) has the same effect as (B + C), then A has the same effect as B.

The substitution postulate is not a necessary condition for the truth of Theorem 2. An alternative proof can be constructed from the assumption that a number α_{A+B} can be defined, which uniquely specifies the response of any given receptor to a mixture of a light A and a light B, and is such that for all lights A and B, the partial derivative $\dfrac{\partial \alpha_{A+B}}{\partial A}$ exists and is not infinite. The proof from this assumption is longer and more difficult than that from the substitution postulate, and will not be given here, since the substitution postulate has a good theoretical basis.

Grassmann's third law

If a light A matches a light B, then (A + C) matches (B + C), where C is any other light; and reciprocally if (A + C) matches (B + C), then A matches B. (Grassmann, 1853.)

The deduction of this law from the substitution postulate requires Theorem 2, and hence would make the proof of that theorem circular. It must therefore be justified experimentally under the conditions of the proposed application of Theorem 2. It is important that Grassmann's third law is required only for the proof of Case 2 of Theorem 2, which comprises a very small fraction of possible instances. Theorem 2 may thus be applied in circumstances where Grassmann's third law is not valid, provided that the application is limited to instances covered by Case 1.

Law of uniqueness of colour matches

If no one of the four lights A, B, C, and D matches the sum of multiples of any two others, then there is only one set of numbers x, y and z such that one of xA, yB, zC and D matches the sum of the other three, or the sum of two of them matches the sum of the other two.

The justification of this law is experimental, not theoretical, and experimentally its validity is clearly limited by the finite size of discrimination steps. It is easy, but tediously lengthy, to introduce modifications in the statement, proof, and applications of Theorem 2 so that discrimination steps are not, as here, assumed infinitesimal. These modifications are omitted, partly to save space, partly because when inserted they make the main arguments difficult to follow through a maze of detail.

Proof of Theorem 2

Case 1

Let A, B, C, and D be four lights such that no one matches the sum of multiples of any two others.

By the postulate of unidimensional response, there must be numbers n_A, n_B, and n_C, such that for all receptors of the first class, n_AA, n_BB, n_CC, and D have the same effect, and numbers p_A, p_B, and p_C, and q_A, q_B and q_C, similarly related to receptors of the second and third classes.

Let x, y, and z be numbers defined by

$$\frac{x}{\begin{vmatrix} 1/n_B & 1/n_C & 1 \\ 1/p_B & 1/p_C & 1 \\ 1/q_B & 1/q_C & 1 \end{vmatrix}} = \frac{y}{\begin{vmatrix} 1/n_A & 1/n_C & 1 \\ 1/p_A & 1/p_C & 1 \\ 1/q_A & 1/q_C & 1 \end{vmatrix}} = \frac{z}{\begin{vmatrix} 1/n_A & 1/n_B & 1 \\ 1/p_A & 1/p_B & 1 \\ 1/q_A & 1/q_B & 1 \end{vmatrix}} = \frac{-1}{\begin{vmatrix} 1/n_A & 1/n_B & 1/n_C \\ 1/p_A & 1/p_B & 1/p_C \\ 1/q_A & 1/q_B & 1/q_C \end{vmatrix}}$$

Then by simple application of the substitution postulate it can be shown that the lights $(xA + yB + zC)$ and D have the same effect on all the receptors, unless one (say z) or two (say y and z) of x, y, and z are negative, in which case the appropriate pair is $(xA + yB)$ and $(D - zC)$ or xA and $(D - zC - yB)$.

We have thus divided A, B, C, and D into two groups such that the sum of multiples of the members of one group has the same effect on all receptors as the sum of multiples of the members of the other. By Theorem 1, this constitutes a match, and by the law of uniqueness of colour matches it is the only match possible between these lights. Hence any two lights which match and can be divided into four components such that no one matches the sum of multiples of any other two must produce the same response in all the receptors.

Case 2

Let E and F be two lights which match, but cannot be divided into four components such that no one matches the sum of multiples of any other two. Let G and H be two lights such that no one of E, G, and H matches the sum of multiples of the other two. Then by Grassmann's third law, no one of G, H, $(E + G)$, and $(F + H)$ matches the sum of multiples of any other two.

Now the lights $(E + G + H)$ and $(F + G + H)$ match (by Grassmann's third law), and they have components, namely G, H, $(E + G)$, and $(F + H)$, which conform to the definition of A, B, C, and D. Therefore, by Case 1 of Theorem 2 $(E + G + H)$ produces the same response in all the receptors as $(F + G + H)$. Therefore, by the substitution postulate, E produces the same response in all the receptors as F.

EXTENSION TO RECEPTIVE PIGMENTS

The theorems and their proofs remain valid if for "class of receptors" we substitute "receptive pigment", and for "response" substitute "amount of photochemical change". It is in this form that the theorems will be used in the two applications which follow.

IRRELEVANCE OF INTERACTION IN THE NEURAL LAYERS OF THE RETINA OR IN THE CEREBRAL CORTEX

If the assumptions made are valid, then the theorems are true, irrespective of the way in which the nerve cells of the visual pathway treat the information passed to them by the receptors. If there are only three kinds either of receptive pigment or of receptors, then these determine that certain lights must match. The proof of Theorem 2 establishes, if the assumptions made are correct, that there are no other colour matches, imposed, for example, by the neural layers of the retina. ·

If, as a result of interaction in neural layers of the retina, activity of one receptor can modify, not merely the activity of nerve cells primarily connected to other receptors, but that of other receptors themselves, this weakens the theoretical basis of one of the assumptions of the proof of the *receptor* form of Theorem 2, namely the substitution postulate; but there is no evidence for such an influence of one receptor upon another, and some electrophysiological evidence against it (Brindley 1956a, b). Even this reservation does not apply to the *pigment* form of Theorem 2.

APPLICATIONS OF THE THEOREMS

Pitt's hypothesis explaining protanomaly

Anomalous trichromats are subjects who, in the red-green range of the spectrum, fail to accept normal colour matches, and make matches which normal subjects do not accept. Pitt (1949) suggested that one class of these anomalous trichromats, the protanomalous, had the normal three receptive pigments, but that their receiving mechanism was such that the spectral sensitivity curve of their red-sensitive mechanism was reduced logarithmically as compared with the normal. Theorems 1 and 2 suffice to disprove this suggestion at once. We choose a colour match which a normal subject accepts but a protanomalous does not. If the normal subject has only three receptive pigments, then by Theorem 2 the two matching lights must affect all of them alike. If the protanomalous has the same three pigments, they must affect them also alike, and therefore by Theorem 1 must match. Since in fact they do not match, the protanomalous subject has not the same three pigments.

The rods as blue receptors

It has several times been suggested (König, 1894; Sivén, 1905; Roaf, 1930; Willmer, 1946) that trichromacy depends on the presence in the retina of one kind of rod, using rhodopsin as its receptive pigment, and two kinds of other receptors, probably cones, each using a single other pigment. Now it is well established (König, 1896; von Kries, 1896; Tschermak, 1898; Stiles, 1955) that if all components of a trichromatic match established for an extra-foveal area of the visual field are attenuated in the same proportion, until no colour vision remains, the match may, if the wavelengths are suitably chosen, conspicuously break down. If the trichromacy depends on the presence of only three receptive pigments, one of which is rhodopsin, then by Theorem 2 the matching lights must affect all these pigments alike, and on proportional attenuation of all their constituents, must by the substitution postulate continue to do so when no colour vision remains, and vision depends on the rhodopsin-containing rods only. Since, in fact, they do not then match, the trichromacy cannot depend on three pigments one of which is rhodopsin.

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MISCELLANEA

A RANDOM PATTERN SCREEN

BY

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Contemporary interest in the part played by surface textures and patterns in visual space perception, and in the rôle of "noise" in visual processes, suggests the desirability of constructing screens bearing random patterns for use in experiments. Of two obvious approaches to this task, one consists in exploiting a natural random process, such as by enlarging photographic grain, or photographing a cathode-ray oscilloscope raster intensity modulated with thermal noise. Alternatively a model might be built up synthetically, using random number sequences, as for instance by punching holes in paper. In theory the first of these methods would appear the more expeditious and practical, but in practice without somewhat elaborate resources technical difficulties intervene, not only in the production of the screen, but also in the control and estimation of its statistical characteristics. The second method requires only simple equipment, and allows virtually complete control of the statistical parameters. It is, however, laborious, and since we have had occasion to produce one such screen in this way, some account of it may be of interest to other workers.

Procedure. The procedure adopted was to fill in or leave blank successive squares on a sheet of 1/12 in. graph paper, 27 in. × 40 in., in accordance with a random series of two digits incorporating the desired statistical distribution. In its original form the screen thus contains approximately 155,000 elements. Pencil was found to be the best and easiest medium for filling in the squares. This original screen was then copied by a photostat process having a non-linear contrast characteristic which results in the dark squares being very greatly intensified, while the faint lines of the graph paper disappear. This intensified screen was then photographed in as contrasty a manner as possible.

It happened that, for the purposes in mind, an equal number of black and white squares was not desired, but instead a black/white ratio of 29 : 71. The distribution is therefore skewed. The necessary random sequence was obtained by a method similar to that used by Kendall and Babington-Smith (1938). A cardboard disc was rotated on a horizontal axis at 2,000 r.p.m. immediately behind a vertical screen with a narrow slit in it radially situated with respect to the disc. The edge of the disc was cut out, by an amount equal to the length of the slit, over an angle of 104° (corresponding to the fraction 0·29). Behind the disc and the slit in the screen was placed a Straboflash lamp set to operate on single flash (duration approximately 12 μs). The operator watched the slit and at intervals, irregular in relation to the speed of rotation, pressed the Straboflash control button. If the light was seen through the slit he recorded (on a typewriter) a symbol for black: if not, another symbol for white. These typewritten records of random sequences from the function $(0.29 + 0.71)^n$ were then used to fill in squares on the graph paper. The inherent coarseness of grain of the screen in relation to that of the retina may be indicated by noting that, if the screen is presented to the human eye on such a scale that each screen-element falls on one retinal element (3μ diameter, say), the whole screen will then occupy a region of the visual field subtending about 2½° × 1¾°.

Figure 1 shows reproductions of portions of the screen on two different scales. The *Statistical parameters.* Five hundred sample squares, each of 100 elements, and 1,000 each of 16 elements were chosen at random from different parts of the complete screen— none of these squares overlapping one another. The distribution for these populations of samples are shown in Fiigure 2, and the means and standard deviations are given, together with the values for the theoretical distributions, in Table 1.

In addition χ^2 was calculated in both cases, the value for $n = 16$ being 12·96 (P >0·15) and for $n = 100$, 26·51 (P >0·15). Thus in neither case is there contradiction of the binomial hypothesis.

FIGURE 1
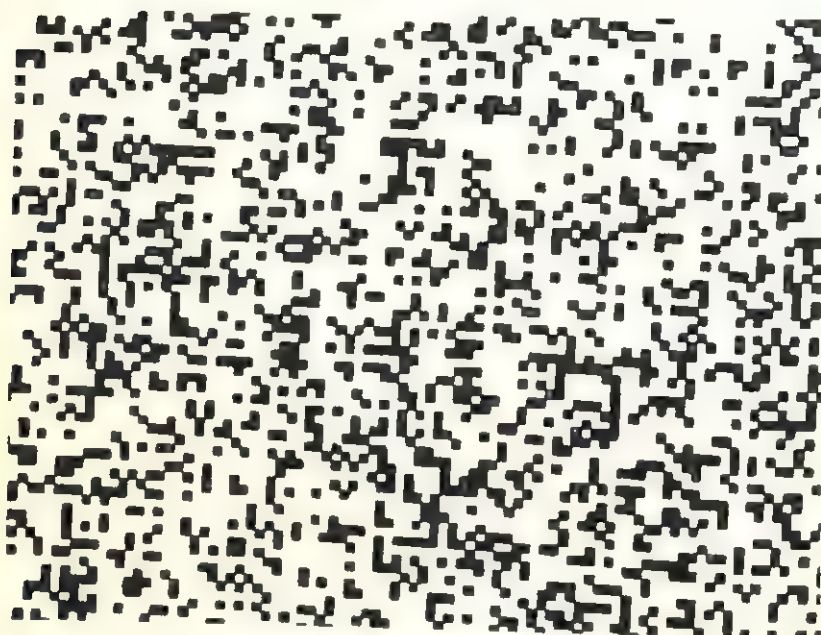
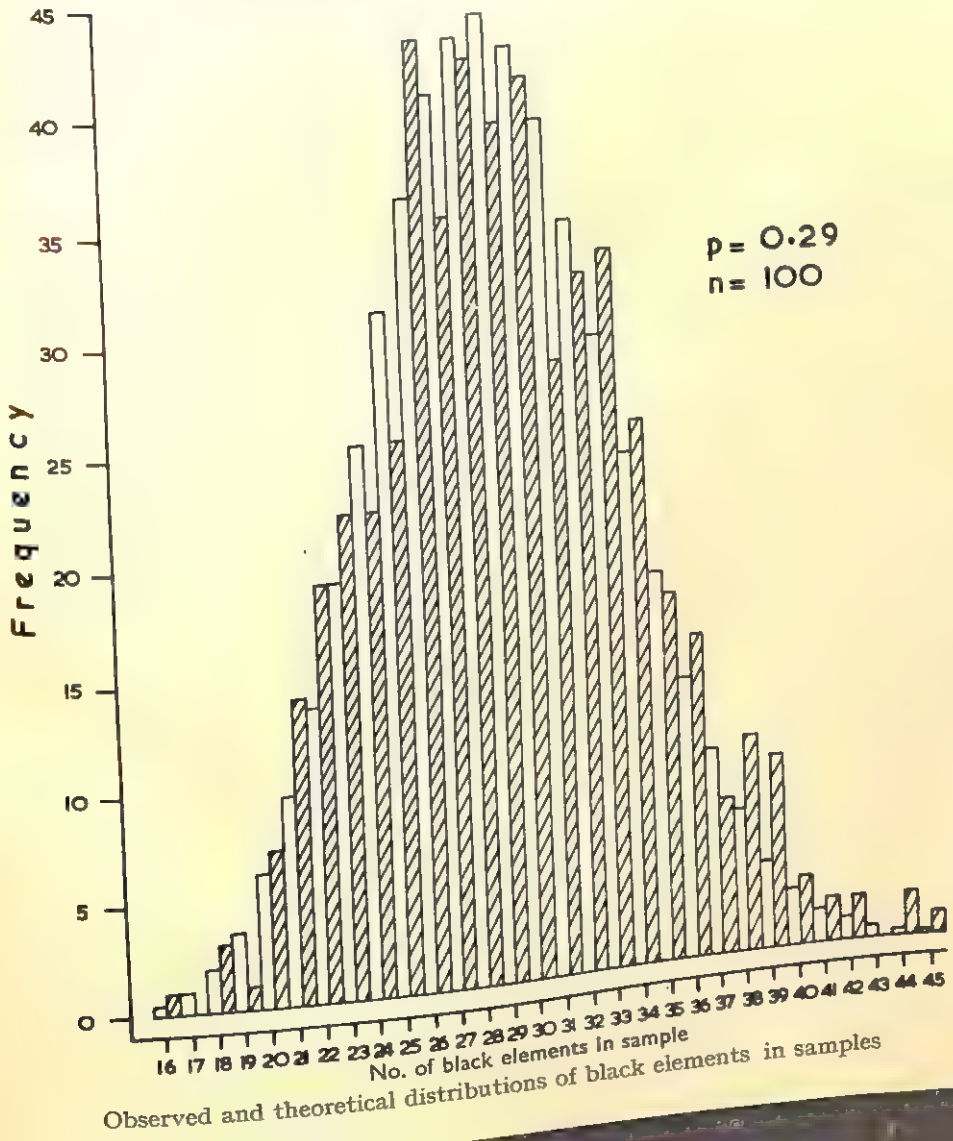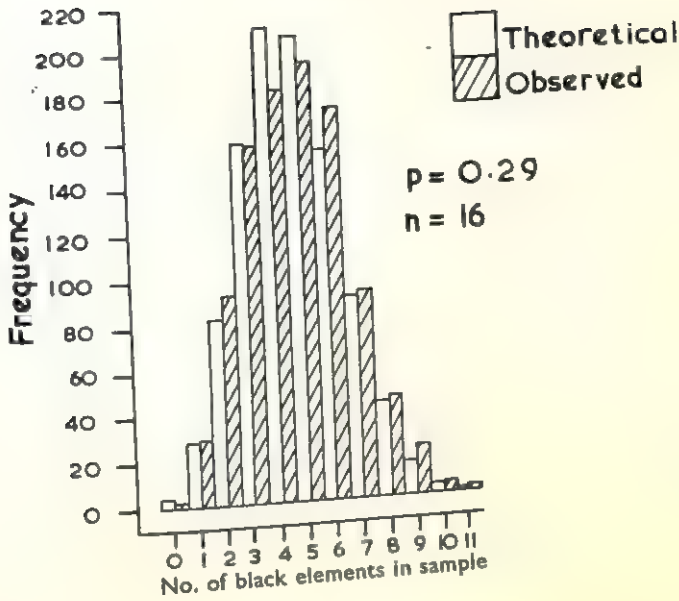Approx. 80 elements per cm²
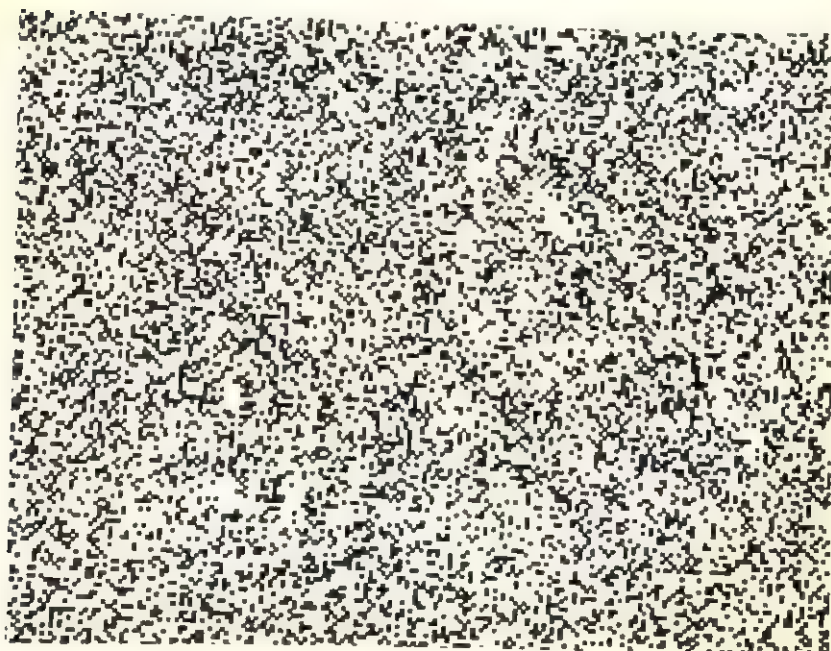
Approx. 200 elements per cm²

FIGURE 2

Observed and theoretical distributions of black elements in samples

TABLE I

		Mean	*S.e. (mean)*	*Stand. dev.*	*S.e. (s.d.)*
$n = 16$	Theoretical	4·64	0·45	1·81	0·32
	Observed	4·73		1·90	
$n = 100$	Theoretical	29·00	0·45	4·54	0·34
	Observed	29·14		4·57	

Equivalent density of the screen. In some experimental applications it may be necessary to know the equivalent density of the screen. It can easily be shown that if, in an area containing n elements, np are light and $nq = n(1 - p)$ dark, and if the densities of light and dark elements are D_1 and D_2 respectively, then the equivalent density is

$$D = D_1 - \log_{10}\{1 - q(1 - 10^{-k})\}$$

where $k = D_2 - D_1 = \log$ contrast ratio.

With any reasonable degree of contrast ($k \geqslant 2$ say), $D = D_1 - \log_{10} p$ to a close approximation, and the equivalent density depends only upon the density of the light elements and the black/white ratio. In both positive and diapositive forms of the screen prepared by us, k was found on measurement to be slightly greater than 2.

If, as in one of our diapositive screens, $D_1 = 0·5$, $k = 2$ and $p = 0·7$, $D = 0·76$. (But it should be borne in mind that D_1 and D_2 may vary considerably in different photographic reproductions, and in particular when these are on different scales.)

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If any reader should wish to obtain a copy of the screen, would he please enquire from Professor R. C. Oldfield, Institute of Experimental Psychology, 34, Banbury Road, Oxford.

DIMINISHING THE ZERO DRIFT IN RECORDING DC FROM THE HUMAN BODY SURFACE

BY

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The electrical A.C. potentials from the human body (ECG, EEG, EMG) have been studied intensively for a number of years. But, apart from the Galvanic Skin Response, D.C. potentials have received relatively little attention. One reason for this difference has probably been the difficulty of preventing the large rapid fluctuations of the zero level, which may arise in the electronic recording system or from the electrode technique, when detecting such small D.C. potentials. Indeed to avoid this difficulty somewhat the Galvanic Skin Response itself has usually been recorded by the resistance method of Féré rather than by the direct potential method of Tarchanoff.

Now that reliable high-gain D.C. amplifiers of adequate stability and sensitivity are more commonplace (Asher, 1951; Liston *et al.*, 1946; Offner, 1954), it is to be expected that interest in recording D.C. from the human body surface will increase. This note is therefore a preliminary communication of electrode and subject preparation techniques which have been found to diminish considerably the zero drifts when recording D.C. potentials in the range from 10 microvolts to 10 millivolts.

There are at present three fields in which these improvements may be of interest. Firstly, Forbes (1936) has emphasized the possible importance of recording the G.S.R. potential by means of an active electrode on one palm with a truly zero neutral electrode on the forearm. Secondly, Köhler (1949, 1951) has shown that there seems to be a D.C. potential change detectable from the scalp surface near the occipital protuberance which is directly related to pattern vision. Thirdly, it is possible to record the position and movement of the eyes by detecting D.C. potentials from the skin surface near the eye sockets (Carmichael and Dearborn, 1948; Marg, 1951); this technique has been given the name Electro-Oculography (EOG).

It was in the course of attempts to improve the EOG technique that it was found necessary to make this study of various possible causes of zero drift in the electrode/skin conductor system. Three major sources of drift or artifact potentials were investigated: (1) the mechanical attachment and chemical purity of practical electrode systems; (2) the G.S.R. potential itself; (3) the Electro-Dermal Response of Ebbecke.

Various types of surface contact electrodes were tried, but none were found satisfactory. A new type of rubber suction cup surface electrode was developed which is just over ½ inch diameter and ½ inch tall, and which adheres well for periods up to at least one hour on all but the hairiest skin. More important, by extreme purity and care in making the Silver Silver-Chloride electrode poles a satisfactory electrical performance has been achieved. A dozen pairs of electrodes have been tested; each pair, in a beaker of 1 per cent. Sodium Chloride, was connected to the high-input impedance recording system for 15 minutes. The maximum drift for any one pair over the whole 15 minutes was 156 microvolts; the mean total drift during 15 minutes of all 12 pairs was 86 microvolts. Each test was divided into 15 one-minute drift sections, and the total drift in each one-minute section was recorded. A percentage distribution of the one-minute drifts, split into 10 microvolt groups, is shown in Table I.

TABLE I

Microvolts	0–10	11–20	21–30	31–40	Above 40
Per cent. of 1-minute drift scores	70·4	23·5	4·3	1·8	0

Another unwanted signal in the EOG situation is the G.S.R. potential itself. It was found, by a controlled test on six subjects, that an intradermal injection of 0·5 c.c. 1 per cent. Xylotox E.100 (a Lignocaine local anaesthetic with one part in 100,000 Epinephrine) diminished the G.S.R. potential from the palm to less than one-tenth the value from the untreated palmar skin. A method more acceptable to subjects of inducing local anaesthesia

of the skin is by electrophoresis (Kovacs, 1949). A simple form of special suction electrode for electrophoresis has been developed.

The Electro-Dermal Response of Ebbecke (Rothman, 1954), a potential difference of up to 10 millivolts elicited by moderate mechanical, thermal or electrical stimulation of the skin itself, is greatly diminished by eroding the outer layers of the epidermis down to, and including, the stratum lucidum and the stratum granulosum; it is not necessary to invade the dermis or draw blood. A dental diamond burr (Dica round burr No. 103) rotating at between 2,000 and 5,000 r.p.m. was found to erode the epidermis easily, quickly, and above all completely painlessly. The polarizing electrical double layer of the skin (which is also the site of most of the skin resistance) is thus breached, with a consequent considerable increase in stability.

With the aid of these techniques it was found, in a test of ten pairs of electrodes *in situ* upon the foreheads of five subjects, that of the zero drifts for one-minute recording periods 72 per cent. were less than 60 microvolts and 91 per cent. less than 100 microvolts.

Full reports of these experiments and techniques will shortly be submitted for publication.

The author wishes to thank the Managing Director and Directors of E.M.I. Electronics Ltd. for their support and encouragement, and for permission to publish these results. The electrodes mentioned in this paper are to be manufactured by Messrs. Phillips & Bonson Ltd., Imperial House, Dominion Street, London, E.C.2.

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DIRECTIONAL INFORMATION AND APPARENT MOVEMENT

A note on the paper by Malcolm A. Jeeves and Jerome S. Bruner
(*Quart. J. exp. Psychol.*, **8**, 107–113)

BY

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In the final paragraph of their paper, Jeeves & Bruner write "We do not, as the reader can see, have a very satisfactory explanation of the findings we have reported. It had been our expectation at the outset that quite opposite results would be obtained, and these might well have been handled in conventional informational terms." I would like to suggest revision of their explanation which, as it stands, only hints at what I would expect to be the most important factor.

In their experiment, they studied the effect of the number of possible positions (directional information) of the second stimulus on whether apparent movement would be seen. The results were:

(1) When the directional information of the second stimulus was high, the transition from apparent movement to succession (or vice versa) occurred at a higher value of the temporal interval, t, separating the two stimuli.

(2) This effect was marked only for ascending series, i.e. series in which t was increased from a small initial value.

(3) Both with high and low directional information, the transition point was at a higher value of t for ascending than for descending series.

If I understand it correctly, their explanation of these results involves three assumptions. The first is that on ascending series subjects have an expectancy for movement. The second is that an expectancy for movement predisposes the subject to see movement. The third is that the effect of expectancy is increased when directional information is high. The first two assumptions will explain results (1) and (2). The third, in conjunction with the other two, will explain result (3).

The first assumption is quite plausible—although *a priori* no more plausible than its opposite, viz. that subjects have an expectancy to *cease* seeing movement as each ascending trial progresses. The second assumption is acceptable as an instance of the principle that a person tends to perceive what he expects to perceive, which is well founded. In support of the third assumption, the authors write "If one distributes one's attention over eight possible pathways of apparent movement, we would hold, this may be tantamount to reducing the kinds of constraints that operate in a field to keep events and points in that field segregated from each other." But it is not clear why distribution of attention should only have this effect when perception is being influenced by expectancy; yet if it has this effect independently of expectancy, result (2) would not be explained. Nevertheless, on other grounds, this assumption too seems to me a reasonable one. For it is established that the effect of expectancy on perception does in general rise with the ambiguity of the stimulus situation and will therefore tend to be greater when directional information is high.

I am dissatisfied with their explanation for two reasons. The first is that, apart from the somewhat puzzling reference to distribution of attention, it leaves out what I would expect to be the main factor. A second and less important reason is that expectancy is arbitrarily assumed to have no effect on descending trials. Yet on descending trials perception may be as much influenced by an expectancy for succession as it is on ascending trials by an expectancy for movement: if it is, result (2) is not explained.

The factor virtually left out is this. There is considerable evidence (see, for example, the work on perception summarized by Miller, 1956) that the threshold for a discrimination tends to increase with the information content of any discrimination which has to be made concurrently. Now a reasonable hypothesis concerning the conditions for apparent movement would seem to be:—Apparent movement tends to be seen if the subject is *able* to

discriminate (d_1) that the two stimuli are not simultaneous but is *unable* to discriminate (d_2) that the two stimuli are not produced by real movement of a single stimulus. In the authors' experiment, a further discrimination had to be made concurrently, viz. the direction (position) of the second stimulus. This should increase the difficulty of making either of the discriminations d_1 and d_2 in proportion to its information content. Accordingly, with increase in the directional information of the second stimulus, both the transition between apparent simultaneity and apparent movement and the transition between apparent movement and succession should occur at higher values of t, the interval between the two stimuli. The second of these deductions would explain result (1): the first remains to be tested. This first deduction is of particular interest, since it conflicts with what is expected on Jeeves & Bruner's theory. For, in relation to the simultaneity/apparent movement transition, expectancy for movement will presumably operate on *descending* trials. Hence, on their theory, directional information will *lower* the value of t for the transition on descending trials and will not affect it on ascending trials (according to the deduction from the theory suggested above, it will tend to *raise* the value of t for the transition, whether on ascending or descending trials).

Once this additional factor is postulated, it becomes possible to explain result (1), without arbitrarily assigning an effect to expectancy on ascending trials only. This is because directional information is now assumed to have two separate effects, one on the difficulty of the discrimination d_2 and the other on the extent to which the subject's expectancy influences what he perceives. Thus on ascending trials, directional information (uncertainty as to where the second stimulus will appear) will delay the transition from apparent movement to succession; firstly, because it increases the difficulty of d_2, and, secondly, because it increases the influence of expectancy for movement. But on descending trials, its effect on d_2 will tend to hasten the transition while its effect on expectancy for succession will tend to delay the transition: the net effect of directional information will here be small, which would explain result (2).

It remains to point out the relation between Jeeves & Bruner's hypothesis of distribution of attention and that of mutual interference between concurrent discriminations. Distribution of attention is simply one possible reason why concurrent discriminations should interfere with one another. Incidentally, such interference is not unexpected on the general hypothesis that rate of gain of information is limited. So it would seem that the results of the experiment *could* be handled in "conventional informational terms" after all.

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THE E.E.G., VISUAL IMAGERY AND ATTENTION

BY

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The claim that persons may be broadly classified into imaginal types on the basis of their E.E.G. records is examined. It is found that a given individual may show a different "type" of E.E.G. from one occasion to the next. A group of persons, known to indulge spontaneously in certain recognizable varieties of vivid visual imagery, contained the same proportion of the different E.E.G. "types" as a large control sample. It was found that subjects could experience changing or static visual images without any blocking of the alpha rhythm providing that difficulty was not experienced in perceiving the images. Flat E.E.G. records were not found to be associated with regular respiration.

It is proposed that the previously reported association between alpha blocking and the appearance of visual imagery could have arisen from the fact that difficulty in thinking (a) activates mechanisms which desynchronize E.E.G. potentials, (b) provokes the emergence of visual images.

I

INTRODUCTION

In recent years it has been proposed that a relation exists between the E.E.G. and imagery employed in thinking. It is claimed that while certain persons habitually employ visual imagery in thinking, others employ verbalization, the latter category being associated with irregular respiration accompanying sub-vocal speech. The "visualizers" are believed to have desynchronized, flat E.E.G. records with no detectable alpha rhythms and the "verbalizers" unusually persistent alpha rhythms (Golla et al., 1943; Golla, 1948; Short, 1953; Walter, 1953; Mundy-Castle, 1951). These proposals have been criticized by Drever (1955) following studies of persons rendered blind early and late in life. In America, Walter and Yeager (1956) found it impossible to estimate the type of imagery used from the character of the electrical activity of the individual blind subject. Barratt (1956) found no evidence that suppression of the alpha rhythm provided a reliable index of the presence of visual imagery; imagery was found to be only one of many factors accompanying suppression.

At the present time it is widely suspected that desynchronization and disappearance of the alpha rhythm is a manifestation of the "level" of alertness, wakefulness, arousal, or attention of the organism, and that it is mediated through an activating mechanism in the brain-stem reticular formation. The evidence has been summarized by Magoun (1954) and by Jasper (1954). Low voltage fast records have been claimed to be particularly common in the presence of anxiety.

Golla, Short and Walter, however, claim that it is activity in those neurones sub-serving visual imagery that is responsible for desynchronization of the alpha rhythm. One worker who previously accepted the relation between imagery and the E.E.G. has, however, lately acknowledged that "the same effect can be caused by an independent factor, such as attention or the state of alertness" (Mundy-Castle, 1955).

The possibility that the association found between introspective reports of imagery and the alpha rhythm, could have an explanation other than that visual imagery causes desynchronization, does not appear to have received due consideration. It has long been realized that visual imagery emerges "at points where our thinking is baffled" (Betts, 1909), "when any slight check occurs, . . . in the absence of this the whole process is likely to run on to completion simply in terms of language" (Bartlett, 1932, p. 220). Humphrey (1951) has discussed the work of Ach and of Fisher, who found that as a mental task became more familiar and less of a problem, visual imagery decreased and verbalization increased.

Therefore it might be proposed that visual imagery may have been reported at the time of flattening of E.E.G. records, because both represented responses to difficulty, with a consequent rise in the "level" of attention, and that when language without visual imagery was reported, it was because there was but little difficulty. This view is consistent with Barratt's finding that where problem solving was accom-panied by alpha suppression, there was a significant decrease in that suppression when the problem was thought over on a second occasion—when it might be presumed to be less difficult. The view that alpha suppression is associated with the level of wakefulness is consistent with the findings of the American workers, Walter and Yeager (1956), that successful recall of visual material was associated with low alpha amplitude during the session when the visual material was presented, the subjects being unaware at the time that they would be asked to recall it later.

Jasper (1954) describes how Einstein was found to show a fairly continuous alpha rhythm while carrying out rather intricate mathematical operations which, however, were fairly automatic for him. The alpha rhythm suddenly disappeared and he reported that he had just thought of a mistake in calculations he had made the day before.

II

OBSERVATIONS

An attempt has been made to follow the classification of E.E.G. records into those with no alpha rhythm, or "M" type, those with alpha rhythms that are normally responsive, the "R" type, and those in which the alpha rhythm is abnormally persis-tent, or "P" type, according to the definitions given by Short (1953). These definitions do not allow of any absolute differentiation between the groups. The common appearance of alpha rhythms just after hyper-ventilation, and briefly, on eye-closure, in those who otherwise lack alpha rhythms has been ignored.

(i) The records of some 200 cases where E.E.G. examination had been repeated after an interval were reviewed. Head injuries and other organic lesions were excluded and among the remainder were found four cases in which on one occasion a patient had had a record which fell clearly into the "M" type and on another occasion clearly in the "R" type. There were several others which, less definitely, could have been similarly classified.

(ii) E.E.G.'s were performed on individuals known to indulge spontaneously in certain particularly vivid varieties of visual imagery and the incidence of "M" types noted.

In 1880 Sir Francis Galton first described individuals who habitually experienced numbers in a particular visual pattern; these images he called "number-forms" and he published a collection of these (Galton, 1883). In the present investigation, out of 57 persons who were found to possess number-forms or analogous "forms" for months, the week, the alphabet or their families, it was possible to examine 33 electroencephalographically. It may be added that not only did these 33 persons experience these "forms," but at least 12 also experienced chromaesthesia and at least 11 could, with eyes closed, perceive so vivid a visual image, that on eye-opening, either a negative after-image or a projection of the original image was experienced with the quality of a real sensation. These latter individuals, and others, have been described elsewhere (Oswald, 1957). There is, therefore, no doubt that amongst the 33 should be found a considerable excess of "visualizers" with "M" type E.E.G.'s, if the existence of this relation were accepted.

In fact, of the 33, only 5 could be said to possess "M" type records. One had a "P" type and the remainder "R" type E.E.G.'s.

In addition a series of initial E.E.G's performed in this department on a consecutive series of 250 patients was examined and, on the same criteria, found to contain exactly 30 "M" type and 4 "P" type records, 4 which were unclassifiable (e.g. a record with generalized rhythms at about 20 cycles per second which blocked with eye-opening), while the remainder were "R" type records.

The incidence of "M" types was therefore practically the same in the group of "visualizers" as in the larger general population. It so chanced that the visual "forms" of the possessors of the five "M" type records were, if anything, poorer than the average in quality.

(iii) With 11 of the 33 "visualizers" described above, it was possible to carry out special E.E.G. examination while they performed various mental tasks involving imagery. In particular they were asked to "see" a certain number, month, etc., on their "forms." It was found that, with some tasks, the alpha rhythm would be momentarily blocked and then return. In other cases no detectable alpha blocking occurred. They had no difficulty in "seeing" these images for as long as required and reported their continuing presence long after any momentary blocking of the alpha rhythm. Thus, there was one subject who possessed a number, month, week, and alphabet-form, who experienced chromaesthesia, and after-images from images, and whose imagery conformed to the criteria of "eidetic" imagery. He was asked to see "101." His alpha rhythm, which was of a fairly continuous 50 microvolts, blocked for 5 seconds and then reappeared and continued without any noticeable difference from the normal for 30 seconds, during the whole of which time he claimed to see "101" in its position. Asked twice during this 30-second period if it was visible, he replied in the affirmative, without effect on the alpha rhythm.

The alpha rhythm of these subjects blocked when they *undertook some new or difficult task*, not while they viewed their visual images. This was further demonstrated in three cases by asking the subject to imagine he was travelling away from the building, along a route he knew well and to visualize the changing scene as he went. There was a momentary blocking of the alpha, after which it persisted steadily while he imaged the changing scene for about half a minute.

It was found that, if asked to perform arithmetical tasks, and to "see" the answer in its number-form position, simple sums did not block the alpha rhythm, though the visual image was present. More difficult sums would block it briefly, but when the answer was achieved and the visual image seen, the alpha returned. This was illustrated by one subject who possessed a number-form consisting of a series of half-loops

(a not uncommon variety). The majority of the possessors of number-forms deny involvement of their forms if asked to multiply 8 by 8, unless specifically asked to see the answer on their forms. This particular subject, however, was one who claimed that he automatically saw "64" in its position. While the record was being taken he was, in any case, asked to see the answer on his form and he affirmed that he did see it. His alpha rhythm, of 30 microvolts, was unaffected by the problem. A request to square 25, however, blocked his alpha rhythm for several seconds.

(iv) The claim that there exists a relation between regularity of respiration at rest, and the resting E.E.G. type, was examined. The principal evidence for such a relation is that of Short (1953).

The records of 100 patients on whom simultaneous E.E.G. and respiratory tracings had been made, were examined. The respiratory tracings were obtained by using a thermocouple near the nose. Some of our records done by this means are technically unsatisfactory and the 100 records were selected from a slightly larger group by an E.E.G. recordist who had no knowledge of the purpose of the investigation, nor why he was selecting 100 technically satisfactory records of respiration. The respiratory tracings were all recorded for one minute, five to ten minutes after the start of the E.E.G. recording. The thermocouple had been previously positioned and even if the patient guessed its purpose (which few do) he had had several minutes to settle down before a respiratory tracing was started, unknown to him.

It was ensured that when the judgement of the regularity or irregularity of respiration was made, there was no possible means of knowing the E.E.G. type and vice versa. A procedure of this type was not, apparently, followed by Short (1953).

Our 100 records were shuffled by assistants who numbered them 1 to 100. They were displayed one by one with only the respiratory tracings visible, the E.E.G. tracing being covered over. The respirations were judged by the writer to be regular or irregular. Another assistant, who alone knew the number of the record, wrote down the judgements. The whole procedure was repeated, again in ignorance of the E.E.G. and of the number of the record. The assistant then selected those records (18 in all) where two inconsistent judgements had been made, and again in ignorance of their numbers and E.E.G.'s, a final decision was made for each. The last 40 seconds of the one-minute's respiratory tracing was examined in each case.

A similar procedure was then carried out with the E.E.G. tracings. Assistants exposed these one by one with only the E.E.G. visible. Records were judged to be "M," "R" or "P." A further category "X," was made for certain records of "R" type, which I believed might have been classed as "M" by, for instance, Golla, who has reported higher incidences of these records than we find. We almost never see "P" records. This is also the experience of the American workers, Walter and Yeager (1956). Our subjects sit in a chair facing the E.E.G. machine and two windows. If they were recumbent, in a plain darkened room, "P" types would no doubt be more common. Only eight of the E.E.G.'s needed a third and final decision as to their type (in seven cases this related to the "X" decision).

The decisions as to regularity of respiration and E.E.G. type had been recorded by an assistant, and not till all were finally made was his record sheet seen by me. The results are shown in Table I.

One E.E.G. which needed a third judgement of "type" had once been tentatively classed as "P." This happened also to be in a person with regular respiration.

TABLE I

RELATION OF E.E.G. "TYPE" TO REGULARITY OF RESPIRATION
IN 100 PATIENTS

	"M"	"X"	"R"	"P"	Totals
Irregular ..	9	7	59	0	75
Regular ..	2	4	18	1	25

III

CONCLUSIONS

(1) The same individual may exhibit different types of E.E.G. record on different occasions. This would appear difficult to reconcile with a typology which would divide subjects into imaginal types on the basis of their E.E.G.'s.

(2) A group of persons known to possess vivid spontaneous visual imagery of a class which may be recognized with a fair degree of objective accuracy, did not contain an excess of flat E.E.G. records.

(3) The presence of a static or changing visual image was not accompanied by desynchronization of the alpha rhythm provided that the person concerned was not experiencing difficulty in achieving that image. Calculation, provided it was easy, did not suppress the alpha in cases where the end product of the calculation definitely involved visual imagery. These facts appear incompatible with the proposal of Golla (1948) that the presence of the alpha rhythm may "be taken as evidence of an arrested or non-cooperative state of the neurones subserving the mental process that is the basis of visual imagery."

(4) The claim that there exists a relation between the resting E.E.G. and resting respiration was not confirmed. There was no excess of regular breathers with "M" or "X" records. The only "P" type had regular respiration, as did the only other which had been tentatively classified as "P." Scrutiny of Short's figures showing α relation between the alpha type and respiration reveal that the relation is dependent on his "P" cases and that he found no significant excess of regular breathers in his "M" group. As previously stated, we see very few "P" records. Golla (1948) stated only that his "M" group showed "the predominantly regular type of respiration." It must be emphasized that all the above considerations of the alpha rhythm and respiration apply to records taken at rest. The writer fully accepts that subvocal speech is associated with irregular respiration. The results noted above suggest that persons with "M" type records may "talk to themselves" just as readily as those with "R" or "P" type records.

(5) Desynchronization of the E.E.G. during thinking occurs when difficulty arises. It is further recognized that difficulty of itself is the common precipitant of visual imagery in thinking. It is therefore proposed that suppression of the alpha rhythm may be explicable solely in terms of mechanisms controlling alertness, without reference to visual imagery. It is recognised that, of the human senses, vision is that which, when stimulated, most readily raises the level of attention.

In conclusion it is necessary to consider the theoretical premises of the belief that there exists an association between the E.E.G. and imagery. These are: (1) That thinking proceeds by the manipulation of images, as Short (1953) seems to believe. (2) That there exist imaginal types.

The first premise is one, the popularity of which has considerably decreased in the last 50 years and would be contested by many, e.g. Woodworth (1915). Humphrey (1951) has reviewed much of the experimental work on this problem.

The second premise is also one which has been doubted. Vernon (1937) wrote, "the evidence for clear-cut visual, auditory, kinaesthetic and verbal types is excessively small." Humphrey (1951) has referred to "the outmoded doctrine of imaginal types."

It is suggested that on both theoretical and experimental grounds, the claim that there exists a correlation between imagery and the E.E.G. merits scrutiny and re-evaluation.

I wish to record how greatly I am indebted to Group-Captain V. H. Tomkins, Principal Specialist in Neuropsychiatry, Royal Air Force, whose encouragement made these observations possible. I wish also to thank the several Electrophysiological Technicians of this department.

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THE HUMAN OPERATOR AS A SINGLE CHANNEL INFORMATION SYSTEM

BY

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A further experiment is reported on reaction times to stimuli separated by short intervals. On this occasion an auditory stimulus was followed by a visual stimulus. Results indicate that the pattern of delays at short intervals is the same as the pattern of delays when the stimuli are presented in one modality only. This suggests a model of the human operator functioning as a single channel through which information from both sense modalities has to pass before appropriate responses are organized. An attempt is also made to reconcile data with the known facts about the peripheral and central components of reaction time and the possibility that delays are the result of occupation of the channel for a central time plus a central refractory time is suggested.

INTRODUCTION

Previous writers have suggested a model for the human operator organizing responses to signals from the environment as a single channel through which one signal had to be cleared before another could be dealt with (Craik, 1947 and 1948, Hick, 1948, Welford, 1952).

The present writer adopted such a model to account for data from experiments on reaction times to consecutive signals at short intervals when it was found that if a second signal was given 0·2 seconds or less after the first signal a lengthened reaction time to the second signal was obtained (Davis, 1956). The amount of the delay increased from zero at Interval 0·25 seconds to approximately 0·15–0·20 seconds at Interval 0·05 seconds and the results were found to fit very closely the formula

Delay = Normal first reaction time—Interval between signals.

The observed second reaction time was then given by

Normal reaction time + Delay.

This confirms the notion of Craik, Hick and Welford on the basic intermittency of function of the human operator (in key pressing situations at any rate), although not supporting Hick and Welford's view that feed-back from a response may occupy the central mechanisms and result in a further delay.

The above experiments have all been performed using sequences of stimuli in the same, the visual, modality. In order to further elucidate the nature of the intermittency it was considered that an experiment similar to that previously reported by the writer might be performed with the difference that the signals following each other should be in different modalities. It was decided in the present experiment therefore to use an auditory signal, a click in a pair of earphones, as the first stimulus and a visual signal, the flash of a neon bulb, as the second stimulus.

Such an experiment should serve to eliminate several possible schemes for the manner in which the human operator is performing. Thus if he is conceived as utilizing only a single channel for the processing of information arriving via any sense modality whatever, then sending in signals via different modalities should not give appreciably different results from sending them in through the same modality. At some point entry to the central channel must be gained and if the signals are too closely spaced "queueing" will occur, with consequent delays in appropriate responses.

(One assumes in all these cases, of course, that signals are not arriving too fast for the actual sensory mechanisms to cope.)

On the other hand the seat of intermittency and "queueing" in the previous experiments may have been more to the sensory side of the arc and there may exist several parallel channels (say one for each modality) each being capable of setting off a response system in relative independence of the others. If then the channels for visual and auditory signals never pass through some common section one might expect that the signals in these two modalities could be dealt with independently (provided they involve different response systems) and no delay due to queueing of signals would occur for any interval between signals.

Alternatively one might argue from work on attention that even greater delays would occur; that giving signals in two different modalities would result in decreased efficiency of the operator, attention having to be transferred from one sphere, the auditory, to another, the visual, and that a subject set to respond to one type of signal would not respond to a different type with maximum efficiency. Work on attention gives no quantitative predictions as to what might happen if shift of attention is the important factor, but one might expect that the delays in the second reaction times would be greater in situations where different modalities were used than in situations where the same modality is used for both signals. The magnitude of such additional delays, obtained by comparing the auditory-visual situation with the visual-visual situation would then give some measure of the time required when shift of attention is involved.

These suggestions, of course, present a greatly oversimplified view of the possible ways of working of the system, but the writer hopes that they are sufficient to show that real alternatives are involved about which experimental evidence may be called upon to decide.

In order to examine these alternatives the following experiment was performed.

Apparatus

The subject sat looking into a cardboard tube, about 7 in. in diameter and 3 ft. long, at the other end of which was mounted a neon bulb. Throughout all the experimental sessions the subject wore padded earphones which served both to provide the auditory signal and exclude extraneous distracting noises. The subject rested the forefinger of each hand on a relay arm which served as a reaction time key. The force required to operate this was small (approx. 80 gm. weight) and, once pressed in, the relay arm was held in by a holding circuit through the relay coil.

The stimuli were provided by a system of condenser-resistance circuits closing relays after certain time intervals, and so arranged in the present experiment to provide 2 signals. The first signal arrived at an interval which could be varied between 1·5 and 2·5 seconds after the experimenter's switch had been pushed (accompanied by the warning "Ready—Now"). The second signal arrived at an interval after the first which could be varied between 50 and 500 milliseconds. The first signal led to a click in the earphones; the second signal produced a flash of the neon bulb, and for this experiment the duration of the flash was fixed at 40 milliseconds.

The subject's task was to press the left key on receiving a click in the earphones and to press the right key on seeing a flash of the bulb.

Recording was done using two sets of Dekatron counters driven by a 1,000 cycle/sec. oscillator. Each relay which provided a signal for the subject also put one set of counters into operation and counting ceased when the subject pressed the appropriate key. Thus reaction times to the nearest millisecond could be obtained.

Experimental procedure

On each day the subject first had to make 15 or so "simple reaction time" responses to the auditory signal alone, with the left hand, followed by 15 or so responses to the visual signal alone with the right hand. The reaction times to the last 10 responses in each set were used as a control series.

The experimental series was then presented. This consisted of 20 pairs of stimuli, an auditory stimulus followed by a visual stimulus. The experimenter would say "Are you ready—Now" and press the switch. At a randomly varied time between 1·5 and 2·5 seconds afterwards the auditory signal would arrive and it would be followed at a randomly varied interval between 50 and 500 milliseconds by the visual signal. The range between 50 and 500 milliseconds was divided into values differing by 50 milliseconds so that in a series of 20 pairs of responses each value of interval would occur twice. The randomness of the arrangement was assured merely by using preselected values determined by drawing from a hat. As in the writer's previous experiment signals were given in pairs, each pair preceded by a clear warning signal, thus providing the best possible conditions for the subject. It was not thought necessary to incorporate any single stimuli (i.e. first stimulus not followed by second stimulus as in the previous experiment). The apparatus made it inconvenient to do this and sufficient check is provided on

(1) the normality of the first reaction—by comparing with the "simple reaction time" controls,
(2) whether the second reaction was being anticipated. If it had been then the longer intervals would have shown a large number of false reactions, i.e. the reaction key would have been depressed before the stimulus had been given.

If in any case reactions to both stimuli in the pair were not made this pair was repeated during the series. This, however, happened rarely and the number of such occasions is shown in the results.

After making each pair of responses the subject was informed of his reaction times. This should tend to make reaction times as short as possible. (Johanson, 1922.)

Before utilizing any of the results obtained each subject was given 5 days' practice in the experimental conditions. A further 20 experimental sessions were then given and the results from these are shown.

RESULTS

Results are given for 2 subjects, one of whom, Subject A had a dominant right hand, the other Subject B having a dominant left hand. This appears to have made little difference.

In Figure 1 are shown the distributions of first (auditory) and second (visual) reaction times for intervals from 50 to 500 milliseconds. Every complete pair of responses is shown and the number of pairs for which, for any reason, a repeat was necessary, is given. (This repetition was used merely for statistical convenience in having the same N of 40 rather than merely giving the number of completed pairs and dealing with slightly different N's.)

From Figure 1 four observations might be made

(1) From intervals of 500 milliseconds, down to 250 milliseconds the distribution of the second (visual) reaction times shows a constant lag behind the distribution of the first (auditory) reaction time. This is to be expected from the well known fact that visual reaction times are normally longer than auditory reaction times. The two sets of distribution for this range correspond well with the control simple reaction times shown in Figure 2. Thus down to 250 milliseconds interval both first and second reactions seem to be performed normally with no interference from the other reaction.

(2) For intervals less than 250 milliseconds a further delay is apparent which increases as the interval gets shorter to a maximum at the shortest interval of 50 milliseconds.

(3) As the interval increases from 50 to 150 milliseconds in 50 millisecond steps the amount by which the second distribution lags behind the first decreases also by steps of approximately 50 milliseconds.

(4) Although the scatter is slightly larger for the second reaction time at the shorter intervals the type of distribution is similar for first and second reaction times at all

FIGURE 1. SUBJECT A.

FIGURE I. SUBJECT B.

Interval 250 ms.
RT₂
N=40
MEAN=176
S.D.=22

RT₁
N=40
MEAN=113
S.D.=15

Interval 200 ms.
RT₂
N=40
MEAN=193
S.D.=31

RT₁
N=40
MEAN=120
S.D.=20

Interval 150 ms.
RT₂
N=40
MEAN=218
S.D.=41

RT₁
N=40
MEAN=116
S.D.=18

Interval 100 ms.
RT₂
N=40
MEAN=255
S.D.=36

RT₁
N=40
MEAN=114
S.D.=12

Interval 50 ms.
RT₂
N=39
MEAN=283
S.D.=31

RT₁
N=40
MEAN=115
S.D.=14

Interval 500 ms.
RT₂
N=40
MEAN=161
S.D.=16

RT₁
N=40
MEAN=113
S.D.=15

Interval 450 ms.
RT₂
N=40
MEAN=162
S.D.=20

RT₁
N=40
MEAN=117
S.D.=18

Interval 400 ms.
RT₂
N=40
MEAN=162
S.D.=21

RT₁
N=40
MEAN=121
S.D.=28

Interval 350 ms.
RT₂
N=40
MEAN=167
S.D.=20

RT₁
N=40
MEAN=117
S.D.=17

Interval 300 ms.
RT₂
N=40
MEAN=166
S.D.=18

RT₁
N=40
MEAN=113
S.D.=16

Distributions of 1st reaction times RT_1 (auditory) and 2nd reaction times RT_2 (visual) for intervals 50 to 500 millisecs. Reaction times to the nearest 1/100th sec. along the horizontal axis. The number of responses falling in each category along the vertical axis. Means and standard deviations are given in millisecs. In the two cases where N = 39 the extreme atypical value has not been used in computing mean and S.D.

Total number of readings requiring repeat for any reason:
Subject A 28 out of 400 = 7 per cent.
Subject B 13 out of 400 = 3·3 per cent.

intervals. Thus the shift in the distribution of second reactions seems the only important difference between results at different intervals.

FIGURE 2
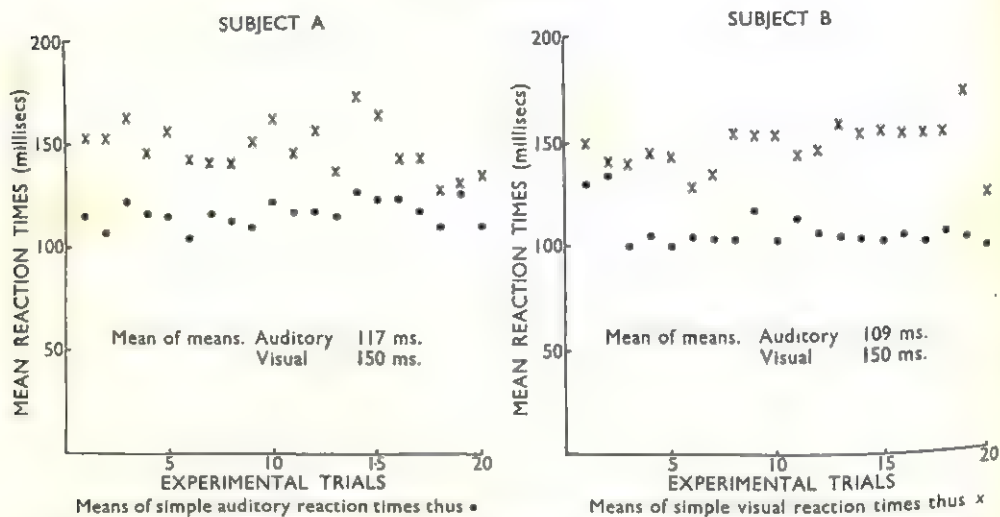
Means of simple auditory and visual reaction times for the last 10 responses in each control series given before each experimental trial

Figure 2 shows the means for the control simple reaction time series in each session. They serve as a standard against which the delays in the experimental conditions can be assessed and also as a check on practice effects. Both subjects had had a certain amount of previous experience in reaction time situations and 5 days' practice on the specific task was given before readings were recorded for use. It can be seen that the level of performance remained approximately the same throughout the 20 sessions—i.e. the effect of practice during the session was slight.

DISCUSSION

The results show a striking overall similarity to those of the writer's first experiment (Davis, 1956) with the difference that the delays at short intervals are superimposed upon the constant lag of the visual reaction times behind the auditory reaction times. This suggests that the same source of delay exists whether signals are given in the same or different modalities, pointing to a single channel conception of the human operator.

There are some slight but suggestive discrepancies in the actual values of delay found.

The original formula, suggested to account for delays when Interval I was less than the reaction time RT_1 to the first stimulus, was Delay $= RT_1 - I$. In the previous experiment no delays were found when $I > RT_1$. In the present experiment the normal RT_1 lay in the region of 110–120 milliseconds, practically all responses falling in the range between 100 and 150 milliseconds (Fig. 1). Thus the formula should predict delays at intervals of 50 and 100 milliseconds, the delays decreasing by approximately 50 millisecond steps to zero delay at Interval 150 milliseconds.

For convenience mean second reaction times of both subjects for all intervals in the experimental series are shown in Figure 3. From this it may be seen that the regular decrease in delay occurs between 50 and 150 milliseconds but that at 150

milliseconds there is still a delay of some 40–60 milliseconds which cannot be accounted for in terms of non-overlapping reaction times. Thus at Interval 150 milliseconds for Subject A all but 7 out of the 40 first responses have been performed and for Subject B all but 3 have been performed. Furthermore, the values of the delay at the two shortest intervals of 50 and 100 milliseconds are about 60 milliseconds longer than the formula would permit. E.g. for Interval 50 milliseconds and $RT_1 = 120$ milliseconds one should predict a delay of $(120 - 50) = 70$ milliseconds. In fact the delays of both subjects are approximately 130 milliseconds.

FIGURE 3
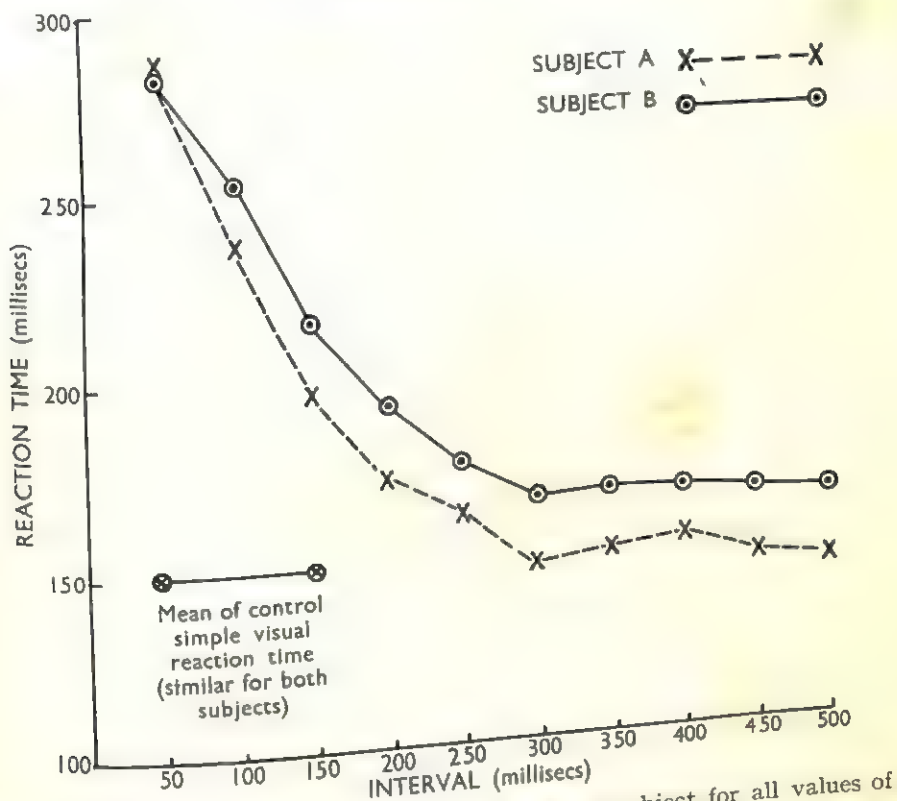
Mean values of 2nd (visual) reaction times for each subject for all values of interval

Is there then a further source of refractoriness? It could be suggested for example that there may be a Refractory Period due to kinaesthetic feedback from the response occupying the central channels. But this was not revealed in the previous experiment where similar reactions were involved (Davis, 1956). (The delays are also rather short to fit the figures suggested by Welford (1952).)

It may be that this further delay, of 60 milliseconds or so, represents the time required for a shift from auditory to visual modalities. However, there may be other alternatives and at this stage perhaps it is worth while to re-examine the original formula, Delay $= RT_1 - I$.

In dealing with delays arising in serial reaction time situations we may assume that blocking occurs in the central mechanisms only, i.e. that peripheral conduction times may very well take place simultaneously. It becomes important therefore,

particularly in dealing with signals from different modalities, to assess how much of the reaction time is a matter of peripheral conduction and how much a central matter.

Values for such peripheral times have been variously estimated as follows.

For an auditory stimulus activity reaches the cerebral cortex 8–9 milliseconds after stimulation (Kemp, Coppée and Robinson, 1937). For a visual stimulus 20–40 milliseconds may elapse before activity reaches the cortex (Bartley, 1934, Marshall, Talbot and Ades, 1943). On the motor side nerve conduction may account for 10–15 milliseconds and the time elapsing before action current is translated into movement of a reaction key has been estimated as 30–40 milliseconds (Vorckel, 1922) (quoted by Woodworth, 1954). This last is probably an overestimation of the time required when a very sensitive reaction key is used. In this case it seems nearer 10–20 milliseconds. Thus, for a response to a visual signal, at least 60 milliseconds or so is spent in the periphery, whereas for an auditory signal the time is much less—about 30 milliseconds.

To deal in terms of gross reaction time would seem unrealistic in the face of this evidence. But why did the results of the previous experiment fit so well the formula Delay = $RT_1 - I$ when only central time should be effective in blocking another stimulus? A feasible suggestion might be that by using the auditory modality, with a consequent shortening of total reaction time and a minimizing of its peripheral components, a further source of delay in the central mechanisms might be revealed. There may exist a truly refractory period following central activity. Thus the central mechanisms may be dealing with the signal for a time equal to (Normal RT − total peripheral conduction time) say 90–100 milliseconds, and this is followed by a truly central refractory time of similar duration, 90–100 milliseconds.

Before applying this analysis to the results it should be noted that a difference in the peripheral times on the sensory side, as occurs when comparing visual and auditory modalities, will result in the onset of the central time occurring relatively sooner or later after the arrival of the stimulus at the sense receptors, and this will have an effect on the amount of delay recorded.

Thus if an auditory signal is given first we may assume that the central mechanisms become occupied within about 10 milliseconds of the occurrence of the stimulus and stay occupied for a period of Central Time + Central Refractory Time, about 180–200 milliseconds.

When a visual signal is given first the central mechanisms do not become occupied until up to 40 milliseconds from the occurrence of the stimulus and they will stay occupied for 180–200 milliseconds so that they do not become cleared again until about 240 milliseconds from the occurrence of the stimulus.

As the delay is assumed to be an entirely central matter, it is quite possible for the peripheral conduction time appertaining to one stimulus to overlap in time with either the central or the peripheral time appertaining to another stimulus. Thus if the first signal is auditory then it can be given about 10 milliseconds before the central channels become cleared and by the time it reaches the central channels there will be no source of delay. If the second signal is visual then it can be given about 40 seconds before clearance without a delay resulting. If both first and second signals are in the same modality the overlapping of peripheral transmission will mean that the total delay will be merely that due to the central mechanisms, viz. Central Time + Central Refractory Time, 180–200 milliseconds. If, however, the first signal is auditory and the second visual this amount will be decreased by the difference between the sensory conduction times for the first and second stimulus, viz. (40 − 10)

—about 30 milliseconds. If the reverse sequence is used then the delays will be lengthened by this amount. Thus the value of delays will be given by

(Central Time + Central Refractory Time) — Interval +

(1st sensory conduction time–2nd sensory conduction time).

The derivation of this formula is shown in Figure 4.

FIGURE 4
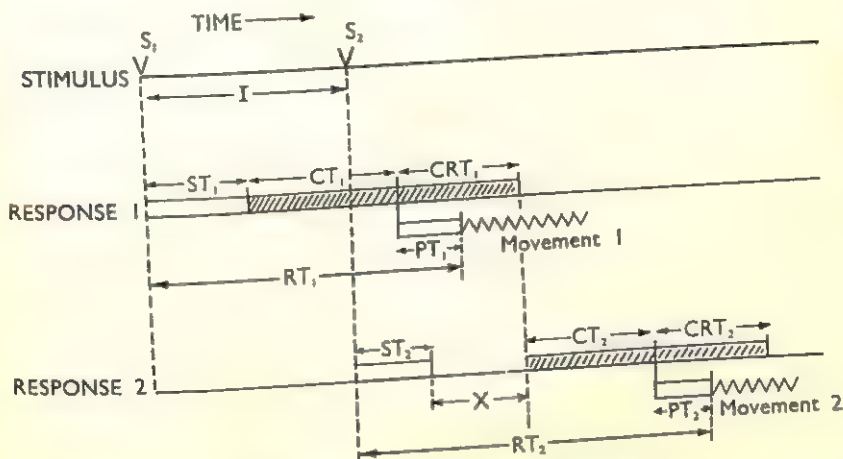
Derivation of the formula for delays

I is the interval between stimuli S_1 and S_2.
RT_1 is reaction time to stimulus S_1.
RT_2 ,, ,, ,, ,, S_2.
ST_1 is sensory conduction time for stimulus S_1.
ST_2 ,, ,, ,, ,, ,, S_2.
CT_1 is central time for stimulus S_1.
CT_2 ,, ,, ,, ,, S_2.
CRT_1 is central refractory time for stimulus S_1.
CRT_2 ,, ,, ,, ,, ,, S_2.
PT_1 is motor conduction time for response 1.
PT_2 ,, ,, ,, ,, ,, 2.
X = amount of delay in the second reaction time.
Thus $I + ST_2 + X = ST_1 + CT_1 + CRT_1$.
Thus $X = CT_1 + CRT_1 - I + (ST_1 - ST_2)$.

In the situation used in the present experiment, viz. auditory signal followed by visual this would give a value of approximately

$$100 + 100 - I + (- 30) \text{ milliseconds}$$
$$= 170 - I \text{ milliseconds}$$

which fits the value of delay found reasonably well.

In the previous visual-visual experiment a different apparatus was used and the control simple reaction times were considerably longer, about 230 milliseconds compared with 150 milliseconds in the present experiment. This is undoubtedly partly a peripheral matter, e.g. the keys were easier to press in the present experiment but there is probably also a central difference—the discriminability of signals was much better in the second experiment. However, it is perhaps still realistic to make a comparison dealing with central times only, and if, say, 90 milliseconds is subtracted for total peripheral time the new formula will predict approximately the same delays as the old for the previous experiment.

Thus Central Time $= 230 - 90 = 140$ milliseconds.
Central Refractory Time $\qquad = 100$ milliseconds.
Delay $= 140 + 100 - I$
$\qquad = 240 - I$ milliseconds

The correction for the difference between 1st and 2nd sensory conduction times being zero. This gives approximately the same result as

$$\text{Delay} = RT_1 - I \text{ where } RT_1 = 230 \text{ milliseconds.}$$

A crucial test for the validity of this analysis would be to perform the experiment using a visual followed by auditory signal sequence, in which case the difference resulting from difference in sensory conduction times will operate in the reverse direction from that in the present experiment, so that the resulting delays ought to be approximately 60 milliseconds greater than delays for the auditory-visual sequence on the same apparatus.

CONCLUSION

The whole of this argument is of course extremely speculative—and we are still left with the possibility that the present result might be explained by an extra delay introduced merely by shifting modalities. The experiment suggested, reversing the present sequence, would be necessary to discount this possibility. It does, however, seem worth while to try and fit the data with the known facts about the breakdown of reaction times into peripheral and central compartments. The postulation of a Central Refractory Time of the order of 100 milliseconds may provide a solution and it is interesting to speculate on the possible relation of this value to the 10 per second periodicity so often noted in rhythmical activity of the cortex which may itself reflect a refractory period of this duration.

Whether or not this analysis is valid the experiment seems clearly to establish that delays of a similar order are likely to occur whether signals are given in the same or different modalities. The fact that "queueing" of signals for central mechanism occurs in both situations suggests that the human operator functions as a single channel through which information from both sense modalities has to pass before appropriate responses are organized.

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THE PERCEPTION OF A ROTATING SHAPE

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A regularly rotated shape was matched to given shapes set in the frontal parallel plane position, under conditions which yielded practically zero constancy. It was found that increases in the rate of rotation resulted in increases in the angular settings at which the subjects matched the rotating shape to the given standard shapes. Thus Langdon's (1951) experimental results were confirmed. However, since matches to the "real" shape also increased with increases in the rate of rotation, Langdon's interpretation of his experimental results is considered to be incorrect. Further theoretical discussion is advanced in support of this conclusion: namely, that regular rotation of a shape, viewed under conditions which usually yield zero constancy, does *not* restore constancy.

INTRODUCTION

In the course of a recent investigation of the phenomena of shape constancy it was found necessary to examine Langdon's (1951) conclusions that "the effect of regular rotatory motion of the shape is sufficient to restore constancy in the continued absence of other cues. Degree of perceptual constancy appears to be correlated with rate of change of shape" (p. 157). Subsequent reports by Langdon (1953; 1955a; 1955b) reiterate this conclusion.

In his first article Langdon offers the tentative explanation that before rotation "the appearance of such a stimulus is purely phenomenal; there is no substantial 'object' . . . the intervention of motion in the manner of regular increase and decrease of the shape, taking place under limited ranges of speed, operates to 'create' the object as a real and subsisting entity" (1951, p. 165). In a later article he states: "regular deformation of the stimulus (due to rotation) provides axes of orientation and enables the outline to be envisaged as occupying the line of regard, thus acquiring the perceptual character of a real, three-dimensional object having spatial orientation" (1955a, p. 26). For Langdon, regular deformation of the stimulus and the consequent endowment of the moving shape with three-dimensionality restores constancy in the continued absence of other cues. But reports from his subjects show that at times they saw "a circle going round" and at other times "a circle being squeezed in and out" (1951, p. 162). (Actually the circle moved from the frontal plane to the line of regard and back to the frontal plane position, i.e., it was not rotated through 360 degrees.) However, variations in the subjects' experience of the moving circle did not "appear to affect their matchings of the shapes" (p. 162). Apparently, regular deformation of the stimulus does not always endow the moving shape with three-dimensionality.

It would appear then, that if Langdon's conclusion is correct (i.e., regular rotatory motion of the shape restores constancy), the restored constancy can occur with or without three-dimensionality appearing in the perception of the moving shape. It may be, however, that this conclusion is not correct. The facts revealed in Langdon's experiments are simply, that the angle of inclination at which the subject matches a rotating circle, viewed in dark space, to a given frontal plane ellipse, is greater than the angular match for stationary shapes under the same conditions, and that this angle increases with increases in the rate of rotation of the rotating circle. Now, if the increase in the angular setting, at which the rotating shape is matched to the frontal plane standard, is due to the introduction of some degree of constancy rather than to other variables, then the appearance of the rotating shape in a frontal plane position, i.e., its "real" shape, should not be affected by variation in the rate of

rotation. If the increases in angular settings, noted by Langdon, were due to other variables, then there would presumably also be a similar increase in the matches to the "real" shape of the rotating stimulus as the rate of rotation increases. Should matches to the "real" shape show an increasing trend similar to those shown by matches to a standard stimulus representing the "real" shape at some angle of slant, then it would be reasonable to suggest, that the same factors responsible for the increasing trend in matches to the slanted shape were also operative in matches to the "real" shape. But constancy factors could not be operative in matches to the "real" shape and so would not appear to be responsible for the increasing trend in matches to the slanted shape.

Experiments were designed to check on Langdon's experimental findings and his conclusions from them.

APPARATUS

To enable regular rotation of the comparison shape an "Electric 12 Recording Drum" (supplied by C. F. Palmer, London, Ltd.) was used. A recording device fitted to the Drum and connected to a key on the right hand arm of the subject's chair recorded the angle of inclination of the comparison shape when he matched it to the standard shape. The standard shape was supported on an upright rod. This rod could be rotated about its vertical axis by the subject by means of a small knob at his left hand side. A pointer, attached to the upright rod, moved over a circular scale, so indicating the angle of slant at which the subject set the comparison to match the standard shape when stationary matches were required.

The test objects were two 4 in. × 4 in. outline squares, one 4 in. × 3·06 in. and one 4 in. × 2 in. outline rectangles made of 24-gauge copper wire and coated with fluorescent powder. They were exposed to ultra-violet radiation from a 250V. 125-watt Osram high-pressure mercury vapour lamp, housed in a metal reflector to which an opal glass filter was attached to reduce the visible light emitted by the lamp.

The subject was seated in a chair of adjustable height and viewed the test objects 10 ft. away, through a reduction tunnel (7 in. long by 1·2 in. dia.) held in a large black felt screen, placed immediately in front of him. The tunnel was 42 in. from the floor and pointed to the midpoint between the horizontal axes of the test objects, whose vertical axes were 8 in. apart. A chin rest was provided to help maintain steadiness of the subject's head. The experiments were carried out in a darkroom 20 ft. × 12 ft. × 10 ft. Looking through the reduction tunnel all that could be seen was the two evenly luminous test objects. Twelve subjects (5 male, 7 female), volunteers and all under-graduate students who had not previously participated in any psychological experiments, were used.

METHOD

Two experimental conditions were investigated. One in which the subject adjusted the slant of a 4 in. × 4 in. comparison shape to match the 4 in. × 4 in., 4 in. × 3·06 in. and 4 in. × 2 in. standard shapes. In the other he pressed a key when the regularly rotating 4 in. × 4 in. comparison shape matched the 4 in. × 4 in., 4 in. × 3·06 in. and 4 in. × 2 in. standard shapes, set in the frontal parallel plane position. Three rates of rotation were used, namely one revolution in 32, 80 and 180 seconds.

The order of presentation of the experimental sessions (i.e., comparison adjusted by the subject and comparison rotating at 32, 80 and 180 seconds per revolution) was randomized for each subject. Presentation of standard stimuli (i.e., 4 in. × 4 in., 4 in. × 3·06 in. and 4 in. × 2 in. shapes set in frontal plane position) within any one session, was also randomized. Each subject made 6 matches (3 with comparison expanding and 3 with comparison contracting) to both the 4 in. × 3·06 in. and 4 in. × 2 in. standards and 4 matches to the 4 in. × 4 in. standard in each of the four experiments.

For stationary matching the main instructions to the subject were: "By turning this knob, adjust the shape on your left until it looks the same as the one on your right. Make some of the matches when the adjustable shape is expanding or going out, others when it is contracting or going in. Take your own time to make as fine a match as you possibly can." Four practice matches were then made.

For rotating matches, the main instructions were to "press this key when the one on your right matches or looks the same as the one on your left. Make a match each time they look the same, that is, as the right one is coming in and as it goes out. If you

miss one of these, don't worry, try to match it the next time. If you do press the key and you think you've been a bit early or late, just say so and I can delete that one from the record. Try to make as fine a match as you possibly can." Four practice matches were made.

RESULTS

The results are presented in Tables I, II and III, which show the mean angles of slant of the comparison shape, matched to each of the three standard shapes in each of the four experiments. The mean frontal parallel plane projection widths of these matches are presented in Table IV.

TABLE I

ANGLE OF SLANT FROM FRONTAL PARALLEL PLANE OF ROTATING COMPARISON SHAPE MATCHED TO THE 4 IN. × 4 IN. STANDARD SHAPE AT 0° (MEAN OF 4 MATCHES)

Subject				Rate of rotation of comparison shape in secs./rev.			
				Stationary	180	80	32
JDF	..	..	..	..	0·25	4·50	4·25	3·00
BRC	..	..	..	..	2·25	3·50	12·00	19·00
EM	..	..	..	..	3·50	10·50	10·75	15·25
CEG	..	..	..	..	1·50	3·25	3·00	4·75
DIR	..	..	..	..	1·00	5·75	4·00	2·00
VD	..	..	..	..	0·25	2·25	3·25	6·75
AT	..	..	..	..	3·00	14·50	17·00	8·50
KK	..	..	..	..	1·00	6·25	4·25	5·50
PC	..	..	..	..	3·50	4·25	2·00	7·00
DL	..	..	..	..	2·00	3·50	4·00	3·25
PB	..	..	..	..	2·00	4·00	5·50	2·25
HVM	..	..	..	..	3·75	4·50	6·75	9·25
Mean	..	..	..	..	2·00	5·56	6·40	6·46

TABLE II

ANGLE OF SLANT, FROM FRONTAL PARALLEL PLANE, OF ROTATING COMPARISON SHAPE MATCHED TO THE 4 IN. × 3·06 IN. STANDARD SHAPE AT 0°. (MEAN OF 3 MATCHES)

Subject			Rate of rotation of comparison shapes in secs./rev.							
			Stationary		180		80		32	
			Comparison		Comparison		Comparison		Comparison	
			In	Out	In	Out	In	Out	In	Out
JDF	..	..	40·50	40·00	47·50	41·00	41·25	43·00	47·50	40·50
BRC	..	..	41·00	41·50	44·00	44·00	43·50	44·00	46·00	43·00
EM	..	..	38·75	38·25	46·00	42·00	42·50	42·50	45·00	42·00
CEG	..	..	41·00	40·50	42·00	46·00	47·00	43·50	47·00	44·00
DIR	..	..	40·25	40·25	43·50	40·00	45·50	45·00	47·00	47·00
VD	..	..	40·00	41·00	41·75	48·25	38·50	39·50	40·00	41·00
AT	..	..	40·00	41·00	44·50	41·50	40·00	42·50	48·00	44·50
KK	..	..	39·50	39·50	38·00	40·00	43·50	45·50	41·00	43·50
PC	..	..	39·75	40·00	41·00	40·00	42·50	39·00	46·00	41·50
DL	..	..	40·00	40·00	43·50	45·00	43·50	44·00	47·00	45·00
PB	..	..	40·00	41·00	40·00	38·50	40·00	39·50	46·00	42·00
HVM	..	..	39·50	39·50	42·00	40·00	45·00	42·50	46·00	45·00
Mean	..	..	40·02	40·21	42·81	42·19	42·73	42·54	45·54	43·25

TABLE III

ANGLE OF SLANT, FROM FRONTAL PARALLEL PLANE, OF ROTATING COMPARISON SHAPE MATCHED TO THE 4 IN. × 2·0 IN. STANDARD SHAPE AT 0°. (MEAN OF 3 MATCHES)

	Rate of rotation of comparison shapes in secs./rev.							
	Stationary		180		80		32	
	Comparison		Comparison		Comparison		Comparison	
Subject	In	Out	In	Out	In	Out	In	Out
JDF	60·50	60·50	62·50	60·00	64·50	62·50	59·50	63·00
BRC	60·25	61·00	63·50	64·00	60·00	60·00	65·00	63·50
EM	59·00	58·50	59·50	64·50	65·00	64·00	64·50	62·00
CEG	59·75	59·50	62·50	63·00	63·50	61·00	65·50	61·50
DIR	60·75	60·25	60·25	58·25	63·50	59·00	62·50	57·50
VD	61·25	61·00	62·50	61·00	61·00	61·00	64·00	62·00
AT	60·50	61·00	61·00	60·00	66·00	59·50	66·00	59·00
KK	58·25	58·25	60·50	59·00	60·50	64·00	62·50	63·50
PC	59·00	60·00	62·00	58·50	62·50	60·00	62·50	58·50
DL	60·00	59·50	64·00	62·50	63·00	63·00	65·00	62·50
PB	60·25	60·75	60·50	59·00	63·00	60·00	64·00	59·00
HVM	59·50	60·00	62·50	60·00	65·50	63·00	64·00	61·00
Mean	59·917	60·021	61·771	60·812	63·167	61·417	63·750	61·083

TABLE IV

MEAN FRONTAL PARALLEL PLANE PROJECTION WIDTHS OF THE ROTATING SHAPE MATCHED TO EACH OF THE STANDARD SHAPES

	Rate of rotation of comparison shapes in secs./rev.							
	Stationary		180		80		32	
	Comparison		Comparison		Comparison		Comparison	
Standard	In	Out	In	Out	In	Out	In	Out
4·0	3·99		3·98		3·97		3·97	
3·06	3·06	3·05	2·93	2·96	2·93	2·94	2·80	2·91
2·0	2·00	1·99	1·89	1·95	1·80	1·91	1·76	1·93

TABLE V

MEAN FRONTAL PARALLEL PLANE PROJECTION WIDTHS OF THE STATIONARY MATCHES TO EACH STANDARD STIMULUS SHAPE

	4 in. × 4 in.		4 in. × 3·06 in.				4 in. × 2·0 in.			
	Angular setting	Projection width	Angular setting		Projection width		Angular setting		Projection width	
			In	Out	In	Out	In	Out	In	Out
	2·00	3·997	40·02	40·21	3·06	3·05	59·917	60·021	2·004	1·996
B.R.					6	0·010			—0·002	0·002

Inspection of the mean frontal parallel plane projection widths of the stationary matches to each of the standard shapes (see Table V) shows the amount of phenomenal regression in each case to be negligible. Modified Brunswik Ratios, calculated from the projection widths of matches to the 4 in. × 3·06 in. and the 4 in. × 2 in. standards are also negligible. Hence it is concluded that the conditions, under which these matches were made, were such that the matches "centre about the point required for fulfilment of the law of visual angle" (Langdon, 1951, p. 159).

TABLE VI

Analysis of Variance of Angle of Slant of Rotating Comparison Shape Matched to Each of the Three Standard Stimulus Shapes at Each of the Four Rates of Rotation When the Comparison Was Moving In

Source of variance	df	Estimate of variance	F
Standard stimulus shape	2	40391·25	6166·60*
Rates of rotation ..	3	137·78	21·04*
Interaction (R × S) ..	6	5·74	0·88†
Within sets	132	6·55	
Total	143		

* P = < 0·01.   † P = N.S.

TABLE VII

Analysis of Variance of Angle of Slant of Rotating Comparison Shape Matched to Each of the Three Standard Stimulus Shapes at Each of the Four Rates of Rotation When the Comparison Was Moving Out

Source of variance	df	Estimate of variance	F
Standard stimulus shape	2	38587·478	5744·749*
Rate of rotation ..	3	62·604	9·320*
Interaction (R × S) ..	6	7·705	1·147†
Within sets	132	6·717	
Total	143		

* P = < 0·01.   † P = N.S.

Two-way classification analysis of variance (Guilford, 1950) of the data in Tables I, II and III, shows significant variation in angular setting for the different rates of rotation (see Tables VI and VII). Single classification analysis of variance of matches to the 4 in. × 3·06 in. and 4 in. × 2 in. standard shapes at each of the four rates of rotation shows all of these (except matches to the 4 in. × 2 in. standard when the comparison was moving out) to yield significant variation with increases in the rate of rotation (see Tables VIII and IX). The present data, then, tend to support Langdon's experimental results.

TABLE VIII

ANALYSIS OF VARIANCE OF ANGLE OF SLANT OF ROTATING COMPARISON SHAPE MATCHED TO THE 4 IN. × 3·06 IN. STANDARD STIMULUS SHAPE AT EACH OF THE FOUR RATES OF ROTATION

Source of variance	Comparison moving in			Comparison moving out		
	df	Estimate of variance	F	df	Estimate of variance	F
Between sets (rate of rotation)	3	60·954	12·462*	3	20·341	4·515*
Within sets	44	4·891		44	4·505	
Total	47			47		

* P = < 0·01.

TABLE IX

ANALYSIS OF VARIANCE OF ANGLE OF SLANT OF ROTATING COMPARISON SHAPE MATCHED TO THE 4 IN. × 2·0 IN. STANDARD STIMULUS SHAPE AT EACH OF THE FOUR RATES OF ROTATION

Source of variance	Comparison moving in			Comparison moving out		
	df	Estimate of variance	F	df	Estimate of variance	F
Between sets (rate of rotation)	3	34·871	14·559*	3	4·268	1·300†
Within sets	44	2·395		44	3·282	
Total	47			47		

* P = <0·01.   † P = N.S.

Table X presents results of analysis of variance of matches to the "real" shape (i.e., 4 in. × 4 in. at o degrees) in each of the four experiments and shows a significant difference between them. The fact of a non-significant interaction between standard shapes and rates of rotation, as shown in Tables VI and VII, indicates that significant differences do not exist between the increases in angular setting of the matches to each of the 4 in. × 3·06 in. and 4 in. × 2 in. standard shapes and the increases in angular setting of the matches to the "real" shape at each of the four rates of rotation. Hence it is suggested that the same factor or factors are responsible in both cases for the increase in angular setting of the matches made at the different rates of rotation.

TABLE X

ANALYSIS OF VARIANCE OF ANGLE OF SLANT OF ROTATING
COMPARISON SHAPE MATCHED TO THE 4 IN. × 4 IN. STANDARD
STIMULUS SHAPE AT EACH OF THE FOUR RATES OF ROTATION

Source of variance	df	Estimate of variance	F
Between sets .. .. (rate of rotation)	3	53·445	4·323*
Within sets	44	12·364	
Total	47		

* P = < 0·01.

DISCUSSION

It appears, then, that matches to the "real" shape vary with increases in the rate of rotation as do matches to given frontal plane projections of the "real" shape, and that the same factors are responsible in both cases. Since shape constancy cannot be said to be present in matches to the "real" shape, it is not present in matches to the frontal plane projections of the "real" shape. Hence it is concluded that regular rotation of a shape, viewed under conditions which usually yield zero constancy, does not restore constancy, even though such rotation does, in fact, result in an increase in the angular setting at which that rotating shape is matched to a given shape set in a frontal parallel plane position.

It is, of course, possible, but highly improbable, that different factors could be operative in matches to the "real" shape as compared with matches to the frontal parallel plane projection of the "real" shape, yet be respectively responsible for the similar increasing trends under the two conditions.

Another reason why the present writer considers Langdon's conclusion to be incorrect arises from a comparison of the task confronting the subject when making rotating matches with his task when making stationary matches. When making stationary matches, he is required to adjust the comparison until it looks the same as the standard shape. Observation of, and reports from, subjects indicate that they adjust the comparison to somewhere near the same shape as the standard, then compare the two, adjust a little, then compare again, and so on. A series of discriminatory responses in terms of the comparison looking the same or otherwise as the standard, are made.

When making a rotating match the subject is required to press the key "when this one looks the same as that one." Since "this one" (i.e., the comparison) is rotating, a reaction time lag is inevitable unless he attempts to anticipate the actual time of apparent equality. If he waits until they both look the same, as the instructions tell him, then, as simple reaction time experiments suggest, some reaction time lag will occur. So the subject's response is based on a momentary discrimination and its recording is contaminated by a reaction time lag. On the other hand, if he attempts to anticipate the actual time of apparent equality, his match is based, not on a series of discriminatory responses made at his own speed of response, but on a time-restricted anticipatory judgement. It is obvious that the two tasks are not "measuring" the same "thing," and direct comparison of the results gained from the two situations is not justified. Since Langdon's conclusion is based on direct comparison of results gained under the two situations, the present writer considers it to be incorrect.

In principle, it would be possible to arrange the two situations in such a way as to justify comparison of results obtained. One way, perhaps, would be to equate the matching time allowed in the two tasks. Instructions to the subject in the rotating match task would need to ensure that he did not anticipate the time of apparent matching. Responses would need to be corrected for individual reaction times. Under these conditions both sets of results would be based on momentary discriminatory responses. Comparison of such results would yield a direct and conclusive check on the conclusions of the present writer.

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INFLUENCE OF A DEPRESSANT DRUG ON ACQUISITION IN ROTE LEARNING

BY

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Central depressant drugs have been shown to impair performance during learning and at recall. The experiment which is described was carried out to investigate whether the effect during learning is primarily on the availability of associations or on their formation. The learning decrement attributable to the drug used (30 per cent. nitrous oxide in oxygen) persisted on recovery from the drug and during subsequent learning under normal conditions. It is concluded that the effect of the drug is on acquisition rather than performance; that is, it acts so as to impede the formative process and does not merely obscure the progress of learning by blocking responses.

INTRODUCTION

The experiment to be described was carried out to investigate whether the formation of associations or their availability at recall was primarily affected by a central depressant drug. It was an essential preliminary to an investigation of retention to be described in a subsequent article.

Investigations of the effect of depressant drugs, like alcohol and nitrous oxide, on recall of material learned under normal conditions consistently indicate impaired recall, e.g., Cattell (1930) for short-term visual memory and responses to general knowledge questions with alcohol, and Seward and Seward (1936) for previously presented syllogistic reasoning problems, again with alcohol. Steinberg (1954) in her investigation of the effects of 30 per cent. nitrous oxide in oxygen on cognitive tasks, found that performance of all tasks was impaired, including tasks depending primarily upon prior associative learning. Thus speed and accuracy of performance of simple arithmetical tasks (addition of two-place numerals) were strikingly affected, and verbal fluency as measured in controlled association tasks was also markedly impaired.

Depressant drugs have similarly been reported to impair performance during learning. McKinney (1932) investigated the effect of 25 per cent. nitrous oxide in oxygen on, among other things, immediate recall of nonsense syllables. He used a method of complete presentation. Though few his data indicate a significant decrement in immediate memory with nitrous oxide. Steinberg's (1954) results included the finding that immediate memory for number series (digit span) was adversely affected. Slower learning of a temporal maze with 30 per cent. nitrous oxide was demonstrated by Russell and Steinberg (1955).

The results of the first kind on recall suggest that an effect of depressant drugs is to reduce the availability of previously formed associations. The results on immediate memory and learning suggest that new associations may be less readily formed. However, in all of the cases cited both the learning and the test performances used to measure it were carried out under the influence of the drug. The possibility therefore remains that the effect in these cases was on the availability of associations at the time of performance (i.e., recall). That this might be the case is suggested by some reports of the effects of depressant drugs on conditioning. Thus Settlage (1936) investigating effects of sodium amytal on conditioned leg flexion in cats, was led to

distinguish a critical state in terms of an optimum dosage at which "the effect of the drug . . . was to inhibit the process underlying the elicitation of the conditioned response without preventing the formation of new stimulus response connections." He indicated that he had obtained similar results with nembutal and alcohol. Sterling and Miller (1941) working with another barbiturate (hexobarbitone) and eyelid conditioning in the cat, also obtained results for some of their animals indicating that "there is a stage at which it is possible to develop conditioning but at which no conditioning is evident, and that the conditioned response can be elicited by the appropriate stimulus after the effects of the drug have worn off." It is possible that depressant drugs might have a similar effect on associative learning in human subjects, but it is on this point that evidence appears to be lacking.

To distinguish effects on learning from effects at recall the requirement evidently is a comparison of recall under normal conditions following learning with and without drugs. If in fact the main effect is on availability of associations at recall, it is to be expected that performance after the effect of the drug has worn off will be comparable with normal performance at the same stage. At the very least a substantially better performance is to be expected than was shown at the conclusion of learning under the influence of the drug; that is to say reminiscence should occur in greater degree following partial learning with drug than following normal learning. If, on the other hand, the effect is on the formation of associations no such difference following learning with and without drug is to be expected; instead the retardation present at the conclusion of learning with drug compared with normal learning should persist. Our experiment attempted to decide between these alternatives. Nitrous oxide—30 per cent. in oxygen—was used as depressant drug with a nonsense-syllable learning task.

Of depressant drugs nitrous oxide is especially suitable for the purpose (cf. also McKinney, 1932) since it is rapidly excreted and subjects return to a normal state almost immediately after withdrawal of the drug. Serial anticipation learning of nonsense syllables was used by Hull (1935a) in his investigation of the effects of the stimulant caffeine on rote learning. His discussion of this experiment (Hull, 1935b) was the starting point of his later mathematico-deductive theory (Hull et al., 1940).

METHOD

Experimental design

The experiment required a learning period, giving equal opportunity for partial mastery of the material, during which air or nitrous oxide were administered. A test of retention at the end of an interval long enough to permit recovery from the drug was then called for. Two groups of subjects were therefore needed:

Group 1—Drug, breathing nitrous oxide during initial learning and air during the interval.

Group 2—Air, as control, breathing air throughout.

Table I shows the difference in treatment of the two groups.

TABLE I

EXPERIMENTAL DESIGN, SHOWING AIR AND DRUG TREATMENTS

Group	Partial learning		Interval (2·5 mins.)	Continued learning Trials 16-40
	Trials 1-4	Trials 5-15		
1—Drug	Air*	Drug*	Air	Air
2—Air	Air*	Air*	Air	Air

* Scented air or drug administered through face mask.

(a) *Partial learning.* Each group underwent serial anticipation learning of the nonsense-syllable task for 15 trials. (It had been ascertained that this number was too few for mastery.) So that any differences in speed of normal learning might be controlled, if necessary, both groups breathed air during the first four of these 15 trials.

(b) *Interval.* When learning was interrupted at the end of trial 15, administration of gases was stopped and the mask removed. Subjects were then occupied in a standardized manner for the remainder of the $2\frac{1}{2}$ minute interval which gave time for recovery from the drug.

(c) *Continued learning.* Anticipation learning was resumed after the interval and terminated as soon as the list was anticipated correctly or at the end of a maximum of 40 trials.

The comparisons primarily of interest were of performances on trials 15 and 16. The prolonged learning after the interval was introduced so that longer term after effects of the differential treatment during trials 5–15 could also be examined. The basic dependent variables used in assessing performances were: (i) correct anticipations: to be counted as correct all letters of the appropriate response syllable had to have been spelled out before the next cue stimulus began, and (ii) failures: absence of response together with partial and erroneous responses of all kinds.

Learning Task

The task to be learned consisted of a series of 15 three-letter nonsense syllables selected from Glaze's list (1928) as having 50 per cent. association values. The syllables were recorded letter by letter on magnetic tape. Each group of three letters occupied two seconds and there was an interval of three seconds between each group. The word "Now" was recorded as initial cue stimulus. The tape was formed into an endless loop and could therefore be used for serial anticipation learning in a manner analogous to the list on an ordinary memory drum. The tape recorder used had a Wright and Weare tape deck for which a special "endless loop" attachment is available. Both the subject and the experimenter listened through earphones. A trial was as usual defined as a complete presentation of the series, and the length of each trial was therefore $16 \times 5 = 80$ seconds.

Administration of gases

Subjects breathed through a face mask connected by rubber tubing to gas flow meters. Those receiving the drug were given 30 per cent. nitrous oxide in oxygen, scented with lavender; the others similarly scented air. The apparatus has been described in detail elsewhere (Steinberg, 1954). The mask was fitted with a microphone in circuit with the experimenter's earphones.

Subjects

Observations were made on 20 male medical or science student volunteers, who were assigned to the two groups at random. The final membership of the drug group was to some extent selected, since six subjects who were originally allocated to it had to be discarded and replaced. Of the six, four ceased to respond while breathing nitrous oxide and two complained of nausea or incipient unconsciousness and were released.

Procedure and instructions

Subjects were familiarized with the earphones and mask and were given the following instructions:

"We want you to learn a series of letters. They will be in groups of three at a time, and you will hear them through the earphones. Just before the series begins each time, you will hear the word 'Now.' The first time through the series you just sit and listen and try to memorize the groups of letters in the series. When you hear the word 'Now' and they begin to come through a second time, we want you to try to anticipate; that is, we want you to try to say each group of letters just before you actually hear it. For example, supposing you have just heard 'A B C,' and you think that the next group is going to be 'X Y Z,' then you say 'X Y Z' quickly in the interval before the next group comes through, and you keep trying to anticipate each

group in this way.   The series will be repeated over and over again until you can anticipate the lot.   If you are not sure what the next group is going to be but have some inkling, try to guess.   Try to learn as actively as you can—don't just sit and wait for it to sink in.   And, of course, the sooner you learn it, the sooner the experiment will be over."

Standard questions were asked to ensure that the instructions had been understood. No warning of any kind was given when Group 1 was switched to nitrous oxide at the end of the 4th trial, and no indication was given that learning would be continued, when it was interrupted after the 15th trial.   The interval of 2½ minutes was occupied with standard questions unconnected with the experiment.   At the end of the interval subjects were instructed to continue trying to anticipate the syllables as they had been doing before.

RESULTS

In Figure 1 the mean number of correct anticipations on each trial has been plotted for each group.   Four aspects of the data are considered: comparability of the groups in initial learning, performance at the end of partial learning under air and under drug, performance after the interval, i.e., after the effect of the drug has worn off, and continued learning.

FIGURE 1

INFLUENCE OF DEPRESSANT DRUG ON ACQUISITION IN ROTE LEARNING
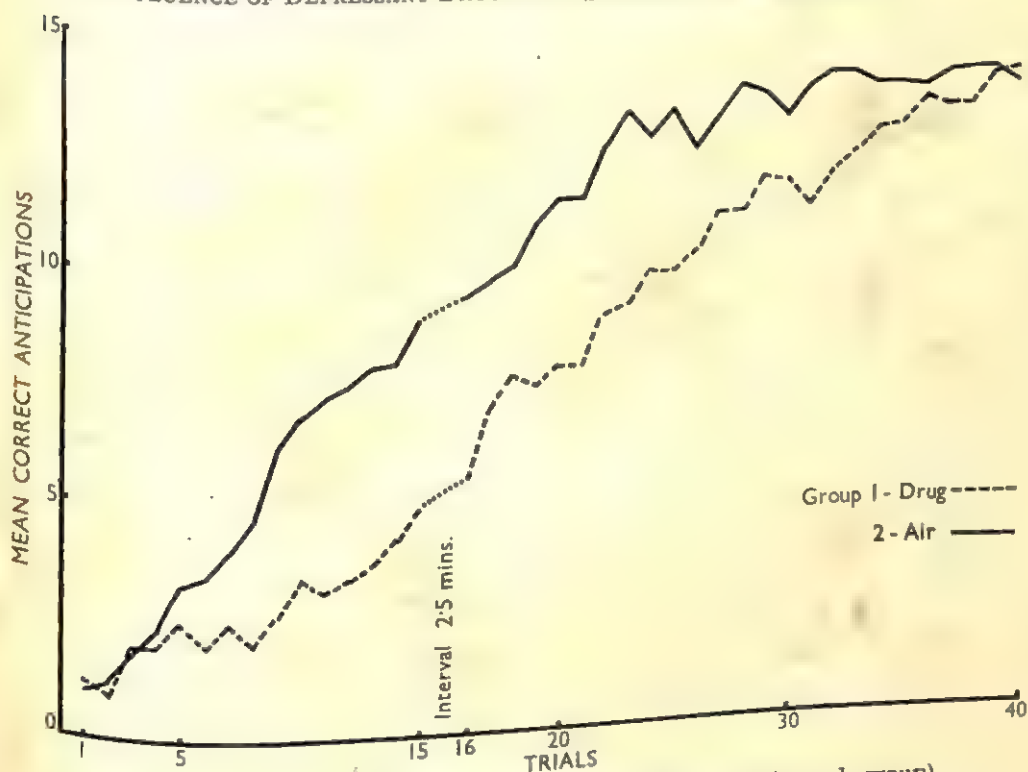
Learning curves for the Drug and Air Groups (10 subjects in each group) plotted in terms of mean number of syllables correctly anticipated per trial.

1.   *Initial learning.*   There are no differences in initial learning between the groups.   Mean and standard deviation values for correct anticipations on trial 4 and for total failures during trials 1–4 are shown in Table II.

TABLE II
INITIAL LEARNING: MEAN AND STANDARD DEVIATION VALUES (a) OF CORRECT
ANTICIPATIONS ON TRIAL 4 (b) OF TOTAL FAILURES FOR TRIALS 1–4

Group	No.	n	(a) Correct anticipations		(b) Total failures	
			Trial 4		Trials 1–4	
			Mean	s.d.	Mean	s.d.
Drug	1	10	2·0	1·41	54·0	4·12
Air	2	10	2·4	1·74	54·0	5·60
Differences			−0·4*		0*	

* P > 0·2.

2. *Partial learning.* The markedly different shape of learning curve over trials 5 to 15 for Group 1 which inhaled nitrous oxide is shown in Figure 1. This group made significantly more failures during the first 15 trials, as is shown in Table III.

TABLE III
PARTIAL LEARNING: MEAN AND STANDARD DEVIATION VALUES (a) OF CORRECT
ANTICIPATIONS ON TRIAL 15 (b) OF TOTAL FAILURES FOR TRIALS 1–15

Group	No.	n	(a) Correct anticipations		(b) Total failures	
			Trial 15		Trials 1–15	
			Mean	s.d.	Mean	s.d.
Drug	1	10	4·8	3·82	184·2	29·28
Air	2	10	8·8	2·79	151·6	29·20
Differences			−4·0		+32·6	
F (df 1 and 18) ..			6·45*		5·59*	

* 0·025 > P > 0·01, single tail.

3. *Performance on recovery from the drug.* As is seen from Figure 1, the difference in performance which was present on trial 15 persisted after the interval, by which time the subjects of Group 1 had recovered from the drug. Mean correct anticipations on trial 16 for the two groups differ significantly, as is shown in Table IV. The

TABLE IV
PERFORMANCE ON RECOVERY FROM THE DRUG: MEAN AND STANDARD DEVIATION
VALUES (a) OF CORRECT ANTICIPATIONS ON TRIAL 16 (b) OF DIFFERENCES IN NUMBER
OF CORRECT ANTICIPATIONS BETWEEN TRIAL 16 AND TRIAL 15

Group	No.	n	(a) Correct anticipations		(b) Differences	
			Trial 16		(Trial 16–Trial 15)	
			Mean	s.d.	Mean	s.d.
Drug	1	10	5·4	3·58	+0·6	1·96
Air	2	10	9·3	3·32	+0·5	1·83
Differences			−3·9		+0·1	
F (df 1 and 18) ..			5·74*		<1·0	

* 0·025 > P > 0·01, single tail.

analysis of the differences in correct anticipations between trials 16 and 15 is also given in Table IV. Both groups show slight improvement over the interval—indicating a tendency to reminiscence; consistently with the results for trials 15 and 16, the amount of improvement is almost identical in the two cases. A test of the difference from zero of the mean improvement for the two groups combined just fails to establish the reminiscence effect as significant: $F = 2 \cdot 90$, df 1 and 19, the tabled value of F at the 5 per cent. 1-tail level being $2 \cdot 99$. The frequencies of no change and improvement *versus* deterioration are:

	No change or improvement	Deterioration
Group 1 (Drug)	6	4
Group 2 (Air)	7	3

4. *Continued learning.* Figure 1 shows that the retardation in acquisition by Group 1 is maintained during subsequent learning, trials 16 to 40. In considering this, it is of course necessary to look at differences along the abscissae between the air and drug groups. The asymptote of the learning curves is necessarily the same for both groups in view of the fixed length of list, and it is for this reason that the ordinates converge. The mean number of correct anticipations on trial 15 by Group 1 was 4·8. The comparable value of 4·7 had already been reached by Group 2 on trial 8, indicating that the retardation in learning produced by the drug was approximately equivalent to seven trials. In Table V mean correct anticipations by Group 1 for trials 15 to 40 are matched against these by Group 2 for trials 8 to 33. The similarity is shown by the small differences. The mean difference of —0·38 indicates that, on the average, the retardation is slightly greater than the seven trials used in constructing the table.

TABLE V

CONTINUED LEARNING: MEAN CORRECT ANTICIPATIONS/TRIAL BY GROUP 1 FOR TRIALS 15–40 MATCHED AGAINST THOSE BY GROUP 2 FOR TRIALS 8–33

Trial No. (Group 1) ..	15	16	17	18	19	20	21	22	23
Trial No. (Group 2) ..	8	9	10	11	12	13	14	15	16
Group 1 (Drug) ..	4·8	5·4	6·8	7·5	7·3	7·7	7·7	8·8	9·0
Group 2 (Air)	4·7	6·2	6·8	7·2	7·4	7·8	7·9	8·8	9·3
Differences	0·1	—0·8	0	0·3	—0·1	—0·1	—0·2	0	—0·3

Trial No. (Group 1) ..	24	25	26	27	28	29	30	31	32
Trial No. (Group 2) ..	17	18	19	20	21	22	23	24	25
Group 1 (Drug) ..	9·7	9·7	10·1	10·9	10·9	11·6	11·5	11·0	11·7
Group 2 (Air)	9·6	9·9	10·8	11·3	11·3	12·4	13·1	12·6	13·1
Differences	0·1	—0·2	—0·7	—0·4	—0·4	—0·8	—1·6	—1·6	—1·4

Trial No. (Group 1) ..	33	34	35	36	37	38	39	40	Av.
Trial No. (Group 2) ..	26	27	28	29	30	31	32	33	
Group 1 (Drug)	12·1	12·6	12·7	13·2	13·1	13·1	13·8	13·9	
Group 2 (Air)	12·3	12·9	13·6	13·4	12·9	13·5	13·8	0	
Differences	—0·2	—0·3	—0·9	—0·2	—0·2	—0·4	0	+0·1	—0·38

Before discussing these results it is necessary to consider the possible consequences of the selection which occurred in the drug group. As has been pointed out, six subjects were released from the experiment owing to their reaction to nitrous oxide. Records of their learning for the first four trials under air were complete. They can

therefore be compared with those who were retained in the drug group for speed of normal learning. Mean total failures for those released were 55·5 compared with 54·0 for Group 2 as shown in Table II. The difference is not significant, and from the absence of relation it is fair to conclude that the learning data for Group 2 are unbiased.

DISCUSSION

The results for partial learning, for performance on recovery from the drug, and for continued learning combine to indicate that the effect of nitrous oxide was primarily to retard the formation of associations, so interfering with the organization of the serial task. This conclusion is reinforced by the finding that the decrement present by the end of partial learning, when the drug was withdrawn, persisted almost unchanged through the interval of $2\frac{1}{2}$ minutes which followed and throughout the subsequent period of continued learning.

The effect on availability of associations at recall appears to have been relatively slight. It is not suggested that there was no effect, and it is possible that reduced recall during serial learning might contribute to a slower rate of learning. But, as discussed at the outset, had the primary effect been on recall it was to be expected that performance of the drug group on recovery would have been substantially better than on the last trial with the drug, owing to the removal of any tendency of the drug to reduce recall. No "post-inhibitory rebound" of the kind reported for conditioning by Settlage (1936) and Sterling and Miller (1941) was, however, found. Such slight increment as there was did not differ significantly from zero, nor was it any greater than in the control group. If associations tended not to be formed under the influence of the drug, it is not surprising that an effect on their availability should be harder to observe; and it is to be expected that an effect of this kind would show itself more readily when not confounded with an effect on the formative process as, for example, when overlearning has occurred prior to recall under the drug. In all the investigations referred to in the introduction it seems likely that overlearning had occurred. The process of "organization" which we suggest is interfered with by the drug is here discussed in terms of association. Kay (1954, 1955) and von Wright (1956) have recently discussed its significance in phenomenal terms in connection with their experiments on problem solving and serial learning of different kinds. Both of these authors have drawn attention to the persistence of early forms of organization in the course of learning, as evidenced by the repetition of errors. In our experiments it is of note that when the drug was given after the fourth trial the level of performance did not fall, indicating the persistence of the early learning and again suggesting that the drug was not acting primarily to inhibit recall.

Gantt and his co-workers (Finkelstein et al., 1945), studying the effects of alcohol on human conditioning, found that: "The ability to form new crs [conditioned reflexes] under the influence of alcohol was much more impaired than was the retention of previously formed crs." Our findings for rote learning are similar. The present experiment indicates that central depressant drugs, in so far as nitrous oxide is typical, act so as to prevent the formation of verbal associations. Impairment of performance during rote learning is not to be explained simply in terms of the blocking of responses by these drugs, i.e., by reduction of the availability of associations which are none the less formed. The former effect predominates, the latter is relatively small.

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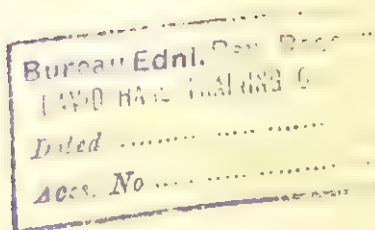
REDUCING INTERFERENCE IN FORGETTING

BY

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According to interference theories forgetting results from the intervention of extraneous associations which disturb the patterns built up during learning. It also seems that the period immediately following learning is critical for forgetting. The experiment which is described was carried out to investigate whether administration of the central-depressant drug nitrous oxide immediately after learning would reduce forgetting, since it had been shown to impair the formation of associations and ought therefore to reduce interference. This expectation was borne out, significantly less forgetting occurring when nitrous oxide was given than when air was given. A possible physiological basis, relating these results to those for sleep is discussed.

INTRODUCTION

The importance of interference in forgetting has been widely acknowledged and reviews of the evidence have tended to conclude, if with reservations, that it must be regarded as the predominant factor. (Hovland, 1951; McGeoch & Irion, 1952; Osgood, 1953; Woodworth & Schlosberg, 1954.) Impairment of performance with passage of time after a given level of mastery has been attained is explained on interference principles as the result of interaction between the state developed during learning and the processes underlying subsequent activities. In the case of verbal learning it has been suggested that subsequent verbal activity (*i*) may produce "reproductive interference" through competition of associations to common or similar elements (McGeoch *et al.*, 1937; Melton & Irwin, 1940), (*ii*) may produce "unlearning" of associations previously formed, through non-reinforcement when they intrude out of context (Melton & Irwin, 1940; Melton & Von Lackum, 1941), and (*iii*) may reduce discriminability of the learned associations (Gibson, 1940). While, therefore, there are important differences of emphasis on the way in which interference may be mediated, all of these approaches carry the implication that extraneous associations intervene to disturb the pattern of associations established by the original learning process (*cf* also Underwood, 1957).

Ebbinghaus (1885) noted that less forgetting occurred from $8\frac{1}{2}$ up to 24 hours after learning than was to be expected from the initial trend in his curves of forgetting. He himself attributed the discrepancy to experimental error, even though he noted that the interval was passed largely in sleep. Jenkins and Dallenbach (1924) followed up the hypothesis Ebbinghaus had rejected. Their experiment, and the confirmatory experiment of Van Ormer (1932), on retention during sleep provide some of the most striking evidence in favour of the interference theory. In both these latter studies some forgetting was nevertheless found to occur. It was, however, negligible from 2 to 8 hours after learning, being concentrated into the early part of the interval and maximal immediately after learning had been concluded. Minami and Dallenbach (1946) reported the same tendency from their study of the effect of induced inactivity on retention in the cockroach; the rate of forgetting was rapidly decelerated, becoming negligible from 2 up to 24 hours. They attributed the forgetting that occurred early in the post-learning period to an anti-consolidation factor which they distinguished from an activity or irritability factor. In their discussion

of other work they consider the experiment of Russell and Hunter (1937) on the effect of sodium amytal on retention of the maze habit in the rat. This experiment is of note in that from 8 to 14 hours' anaesthesia immediately following learning was found to result in no saving compared with a normal control group when retention was determined 24 hours after learning. In addition, retention in the experimental group was not found to be correlated with duration of anaesthesia, which varied for different animals. As Minami and Dallenbach (1946) observe, "the authors [Russell and Hunter] did not, however, consider their results as conclusive because of the possibility, since the control rats seemed to have a stronger thirst drive than the experimental groups, that the amytal animals were still physiologically affected by their anaesthesia at the time of relearning." Neither Russell and Hunter nor Minami and Dallenbach did, however, consider another possibility which emerges from the data for the control animals in Minami and Dallenbach's own study: such forgetting as occurred over 24 hours in the control group was virtually complete after the first eight hours. At least eight hours elapsed between the end of anaesthesia and the test of retention for all of Russell and Hunter's experimental animals (except one which showed maximum retention), and this period may have been sufficient for them to drop to much the same level of retention as the control group by the time the test was made.

The evidence suggests therefore that the period immediately following learning may be critical for forgetting. Our finding, reported in the preceding paper (Steinberg & Summerfield, 1957), that the central depressant drug nitrous oxide acted to impede the formation of associations, implies that the administration of such a drug ought to reduce forgetting, in so far as forgetting may be the result of interference via intervening extraneous associations. Furthermore, it would appear essential to use a drug the effects of which are rapidly dissipated if the difficulties of Russell and Hunter's experiment are to be avoided. The action of barbiturates like sodium amytal tends to be prolonged, but as was observed in our earlier article this is not the case with nitrous oxide, especially when used in sub-anaesthetic doses (cf. also McKinney, 1932). The experiment which is now described was therefore carried out to investigate the effect of administering a low concentration of nitrous oxide (30 per cent. in oxygen) during a period of ten minutes immediately following partial learning of a nonsense-syllable learning task.

METHOD

Experimental design

The experiment required a learning period for attainment of partial mastery of the material, followed by an interval before retention was tested with air or nitrous oxide administered during this interval. Basically, therefore, two groups of subjects were called for:

Group E_1—air/drug, breathing air during initial learning and nitrous oxide during the interval;

Group C —air/air, as control, breathing air throughout.

A second experimental group was also included to provide an additional basis of comparison:

Group E_2—drug/drug, breathing nitrous oxide throughout.

Table I shows the differences in treatment of the three groups.

(a) *Partial learning.* Each group underwent serial anticipation learning of the nonsense syllable task for 15 trials. To enable any differences in initial learning ability to be assessed all three groups breathed air during the first four of the 15 trials.

(b) *Interval.* Learning was interrupted at the end of trial 15. A colour naming task was used to occupy the subjects during the first 10 minutes of the interval, at the end of

Table I

Experimental Design, Showing Air and Drug Treatments

Group	Initial learning (Trials 1–4)	Partial learning (Trials 5–15)	Interval (12.5 mins.)	Continued learning (Trials 16–40)
C-Air/Air	Air*	Air*	Air*	Air
E₁-Air/Drug	Air*	Air*	Drug*	Air
E₂-Drug/Drug	Air*	Drug*	Drug*	Air

* Scented air or drug administered through face mask.

which inhalation through the mask was stopped and the mask was removed. Standard questions unconnected with the experiment were put to all subjects during the remaining 2½ minutes.

(c) *Continued learning.* Anticipation learning was resumed after the interval and terminated as soon as the list was anticipated correctly or at the end of a maximum of 40 trials.

The comparisons primarily of interest were of performances on trials 15 and 16, immediately before and after the interval. The prolonged learning after the interval enabled longer term after-effects of the differential treatment prior to trial 16 to be examined. The basic dependent variables used in assessing performances were: (i) correct anticipations, and (ii) failures. These terms have been defined on p. 140.

It will be seen that the sequence of events was similar to that of the previous experiment, except that the interval was longer and during it the drug was given to some subjects.

Learning task. The nonsense syllable learning task was identical with that used in our previous experiment (p. 140).

Colour naming. Colours arranged in random order were presented at a rate of 75 per minute, by means of coloured squares attached to a large diameter drum which revolved behind an apperture.

Administration of gases (cf. p. 140).

Subjects. Observations were made on 30 male medical or science student volunteers, who were assigned to the three groups at random. The final membership of group E₂ was to some extent selected, since 7 subjects who were originally allocated to it had to be discarded and replaced. Of the 7, 5 ceased to respond while breathing nitrous oxide and 2 complained of incipient unconsciousness and were released.

Procedure and instructions

Subjects were familiarized with the mask and earphones and were given the following instructions:

"We are going to ask you to carry out two tasks. The first one is to learn a series of letters. [The instructions which followed were identical with those on p. 140.] The second task involves this apparatus in front of you. You will see some coloured squares passing behind this window. As they pass, we want you to say what the colours are, one by one. Don't worry about finer shades, but just give the main colour names."

Standard questions were asked to ensure that the instructions had been understood. No warning of any kind was given when Groups E₁ and E₂ were switched to nitrous oxide at the end of trail 15 and trial 4 respectively, and no indication was given that learning would be continued when it was interrupted after trial 15. The procedure followed during the interval of 12½ minutes has already been described above. At the end of the interval subjects were instructed to continue trying to anticipate the syllables as they had been doing before.

Results

In Figure 1 the mean number of correct anticipations on each trial has been plotted for each group. Four aspects of the data are considered: comparability of the groups in initial learning, performance at the end of partial learning, retention over the interval, and continued learning.

FIGURE I
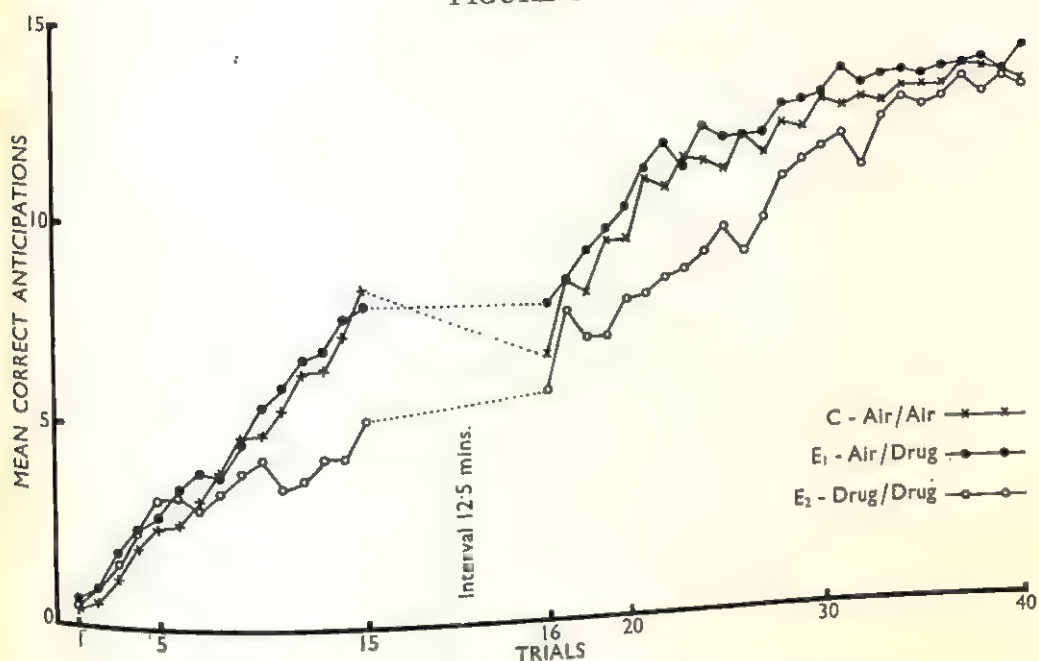
Learning curves for the Air/Air, Air/Drug and Drug/Drug Groups (10 subjects in each group) plotted in terms of mean number of syllables correctly anticipated per trial.

1. *Initial learning.* There are no differences in initial learning between the groups. Mean and standard deviation values for correct anticipations on trial 4 and for total failures during trials 1–4 are shown in Table II.

TABLE II

INITIAL LEARNING: MEAN AND STANDARD DEVIATION VALUES (a) OF CORRECT ANTICIPATIONS ON TRIAL 4 (b) OF TOTAL FAILURES FOR TRIALS 1–4

Group	n	(a) Correct anticipations Trial 4 Mean	s.d.	(b) Total failures Trials 1–4 Mean	s.d.
C—Air/Air	10	2·0	1·41	55·8	2·89
E$_1$—Air/Drug	10	2·5	1·50	53·9	2·39
E$_2$—Drug/Drug	10	2·4	1·20	54·5	3·26
F (df 2, 27)		<1·0		1·03	

2. *Partial learning.* The similarity in the course of learning over trials 1–15 for Groups E$_1$ and C, both of which breathed air, is shown in Figure 1. There are no significant differences between the groups in correct anticipations on trial 15 or in total failures during the first 15 trials, as is shown in Table III. Both measures of performance show the expected difference between Group E$_2$, which breathed nitrous oxide, and the combined results of Groups E$_1$ and C.

TABLE III

PARTIAL LEARNING: MEAN AND STANDARD DEVIATION VALUES (a) OF CORRECT
ANTICIPATIONS ON TRIAL 15 (b) OF TOTAL FAILURES FOR TRIALS 1–15

| | | (a) Correct anticipations | | (b) Total failures | |
| | | Trial 15 | | Trials 1–15 | |
Group	n	Mean	s.d.	Mean	s.d.
C—Air/Air	10	8·4	3·07	165·8	24·62
E$_1$—Air/Drug ..	10	8·0	3·44	160·8	28·60
F (df 1, 18) ..		<1·0		<1·0	
C + E$_1$	20	8·2	3·26	163·3	26·80
E$_2$—Drug/Drug ..	10	5·2	2·09	178·0	19·05
F (df 1, 28) ..		6·54*		3·29†	

* 0·025 > P > 0·005, single-tail.
† 0·05 > P > 0·01, single-tail.

3. *Retention over the interval.* Figure 1 shows that, while there was a deterioration in performance from trial 15 to trial 16 in Group C, which breathed air during the interval, there was no such deterioration in Groups E$_1$ and E$_2$ which breathed nitrous oxide. Mean and standard deviation values of differences in number of correct anticipations between trial 16 and 15 are shown in Table IV. An analysis of variance

TABLE IV

RETENTION OVER THE INTERVAL: MEAN AND STANDARD DEVIATION
VALUES OF DIFFERENCES IN NUMBER OF CORRECT ANTICIPATIONS
BETWEEN TRIALS 16 AND 15

| | | | | Differences | |
| | | | | (Trial 16–Trial 15) | |
Group			n	Mean	s.d.
C—Air/Air ..	..	..	10	−1·8	1·48
E$_1$—Air/Drug	..	..	10	−0·1	1·70*
E$_2$—Drug/Drug	..	..	10	+0·5	0·92*
F (df 2, 27)	..	..		6·51†	

* The three variances are homogeneous: S² max./S² min. = 3·40
    P > 0·05 (cf. Pearson and Hartley, 1954, p. 60).
† 0·01 > P > 0·001.

applied to all three groups indicates reliable differences among these means (F = 6·51, df 2 and 27; 0·01 > P > 0·001). In particular (a) the difference between Groups E$_1$ and C, which received exactly the same treatment before the interval is reliable at the 1 per cent. level, against the overall error variance (F = 6·61, df, 1, 27; 0·01 > P > 0·001); (b) the difference between Groups E$_1$ and E$_2$ both of which breathed nitrous oxide during the interval, is not significant (F < 1). Comparing Group C with Groups E$_1$ and E$_2$ in terms of whether there was no change or improvement, or whether there was deterioration in performance over the interval, gives the following frequencies:

	No change or improvement	Deterioration
Group C (Air)	2	8
Groups E$_1$ and E$_2$ (Drug) ..	15	5

Applying the Fisher-Yates exact test for a 2 × 2 contingency table (Latscha, 1953) shows the difference between the groups to be significant (P = 0·012). The tendency to reminiscence in Group E_2 is not significant (F = 1·14, *df* 1 and 27; P > 0·25). Obliviscence in Group C is highly significant (F = 13·25, *df* 1 and 9; 0·01 > P > 0·001).

4. *Continued learning.* Figure 1 suggests that the advantage in retention of Group E_1 over Group C after the interval is maintained during subsequent learning. In Table V mean correct anticipations by Group E_1 for trials 16–38 have been matched against those by Group C for trials 18–40. The small differences (average +0·003) indicate that the advantage is equivalent to about two trials. The similar comparison of Groups E_1 and E_2 in Table VI confirms the result of our earlier experiment by indicating a retarding effect of nitrous oxide on learning equivalent to some six or seven trials.

TABLE V

CONTINUED LEARNING: MEAN CORRECT ANTICIPATIONS/TRIAL BY GROUP E_1 FOR TRIALS 16–38 MATCHED AGAINST THOSE BY GROUP C FOR TRIALS 18–40

Trial No. (Group E_1)	16	17	18	19	20	21	22	23
Trial No. (Group C)	18	19	20	21	22	23	24	25
Group E_1	7·9	8·5	9·2	9·7	10·2	11·1	11·7	11·1
Group C	8·1	9·4	9·4	10·8	10·6	11·3	11·2	11·0
Differences	−0·2	−0·9	−0·2	−1·1	−0·4	−0·2	0·5	0·1

Trial No. (Group E_1)	24	25	26	27	28	29	30	31
Trial No. (Group C)	26	27	28	29	30	31	32	33
Group E_1	12·1	11·8	11·8	11·9	12·6	12·7	12·9	13·5
Group C	11·8	11·4	12·1	12·0	12·7	12·5	12·7	12·6
Differences	0·3	0·4	−0·3	−0·1	−0·1	0·2	0·2	0·9

Trial No. (Group E_1)	32	33	34	35	36	37	38	Av.
Trial No. (Group C)	34	35	36	37	38	39	40	
Group E_1	13·1	13·3	13·4	13·3	13·5	13·5	13·7	
Group C	13·0	13·0	13·0	13·5	13·4	13·3	13·1	
Differences	0·1	0·3	0·4	−0·2	0·1	0·2	0·6	+0·003

TABLE VI

CONTINUED LEARNING: MEAN CORRECT ANTICIPATIONS/TRIAL BY GROUP E_1 FOR TRIALS 9–34 MATCHED AGAINST THOSE BY GROUP E_2 FOR TRIALS 15–40

Trial No. (Group E_1)	9	10	11	12	13	14	15	16	17
Trial No. (Group E_2)	15	16	17	18	19	20	21	22	23
Group E_1	4·6	5·5	6·0	6·7	6·9	7·7	8·0	7·9	8·5
Group E_2	5·2	5·7	7·7	7·1	7·1	7·9	8·0	8·4	8·6
Differences	−0·6	−0·2	−1·7	−0·4	−0·2	−0·2	0	−0·5	−0·1

Trial No. (Group E_1)	18	19	20	21	22	23	24	25	26
Trial No. (Group E_2)	24	25	26	27	28	29	30	31	32
Group E_1	9·2	9·7	10·2	11·1	11·7	11·1	12·1	11·8	11·8
Group E_2	9·0	9·6	9·0	9·8	10·8	11·2	11·5	11·8	11·0
Differences	0·2	−0·1	1·2	1·3	0·9	−0·1	0·6	0	0·8

Trial No. (Group E_1)	27	28	29	30	31	32	33	34	Av.
Trial No. (Group E_2)	33	34	35	36	37	38	39	40	
Group E_1	11·9	12·6	12·7	12·9	13·5	13·1	13·3	13·4	
Group E_2	12·2	12·7	12·5	12·7	13·2	12·8	13·2	13·0	
Differences	−0·3	−0·1	0·2	0·2	0·3	0·3	0·1	0·4	+0·08

It is again necessary, as in our first experiment, to consider the possible influence of selection in Group E_2. Seven subjects originally assigned to this group were released from the experiment owing to their reaction to nitrous oxide. Mean total failures during trials 1–4 for those released were 54·43, compared with 54·5 for those retained in Group E_2. The absence of difference suggests that the data for Group E_2 are unbiased.

DISCUSSION

The expectation, entertained at the outset, that administration of nitrous oxide during an interval in learning would retard forgetting is borne out by the results for performance before and after the interval, both in terms of the analysis of differences and in terms of the fairly constant retardation—two trials—by which Group C lagged behind Group E_1 during continued learning. The effect on retention over the interval appears to be independent of level of acquisition, since there is no difference in retention between Groups E_1 and E_2, in spite of the difference in performance by trial 15; again, the results for continued learning indicate that the extent to which Group E_2 was lagging behind Group E_1 by trial 15—equivalent to between six and seven trials —persisted during continued learning. Some forgetting evidently occurred in all three groups since performances on trial 16 are inferior to those which would be expected with uninterrupted learning. The rate of forgetting in our control group is of the same order as that found by Jenkins and Dallenbach (1924) for the waking state. There is, however, a marked difference between the flat trends shown by our experimental groups and the initial decline in Jenkins and Dallenbach's data for sleep. As McGeoch and Irion (1952) observe, however: "Part of the initial decline may be a function of the activities of leaving the experimental room, going to bed and getting to sleep." The same sharp initial decline also appeared in Van Ormer's results (1932). It would be of interest for these experiments to be repeated with use made of the technique employed by Simon and Emmons (1956) for monitoring depth of sleep, and also with deep sleep induced artificially by sedation. These differences in detail notwithstanding, the effect of nitrous oxide remains consistent with the hypothesis that it acted so as to reduce interference and enhance retention in a manner similar to the overall effect of sleep following learning.

The similarity between the effects of an anesthetic drug and those of sleep in this context provokes speculation in view of recent physiological developments. The part played by brain stem structures in central nervous functions underlying consciousness is being more and more emphasised (Penfield, 1955; Adrian, Bremer & Jasper, 1954). It has in particular been shown that the "reticular activating system" is concerned in regulating sleep and wakefulness and is also especially sensitive to the effects of anaesthetics. The functions ascribed to this system have recently been summarized by Lindsley (1956) and by Jasper, Gloor and Milner (1956). Some of its heuristic possibilities have been discussed by Hebb (1955). The system comprises mid-brain structures together with ascending pathways through the diencephalon which connects them diffusely with the cortex. At the level of the mid-brain it receives collaterals from the classical sensory pathways as these ascend via the thalamus to the primary projection areas. Stimulation of sense organs produces activity in the system (French, Verzeano & Magoun, 1953a). In drowsiness and sleep activity is depressed or blocked, and it has repeatedly been shown, since the first observations of Moruzzi and Magoun (1949), that the onset of activity is associated with cortical arousal. Anaesthetics have also been shown to depress activity in this system as one of their first effects on the central nervous system (French, Verzeano & Magoun, 1953b), while leaving impulses passing via the classical routes

unimpaired or augmented; impulses do indeed continue to reach the primary projection areas of the cortex in deep anaesthesia (Marshall, Woolsey & Bard, 1941), as also in deep sleep. In considering associative mechanisms under conditions of sleep and anaesthesia, it is also of note that Simon and Emmons (1956) found no evidence that learning occurred during deep sleep. Support for the supposition that the reticular system is involved in interference effects in associative learning comes from recent work at McGill by Dr. Helen Mahut (*personal communication*). She has found that direct electrical stimulation in the following regions produces significant deficits in learning by rats (Hebb-Williams test): the brain reticular formation, the region of the thalamic intralaminar nuclei, and, subject to necropsy, the dorsal hippocampus.

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METHOD OF SERIAL EXTRAPOLATION

BY

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A psychophysical procedure is proposed for measuring a subject's power of discerning "lawfulness," "regularity," "invariance" in a stimulus sequence. The task consists of supplying the next term in a given series. The method is tested by applying it to a visual series consisting of lines subdivided in geometric progression. The effect upon accuracy of judgement is investigated by varying the settings of the stimulus, in particular the rate of increase or gradient of the series and the number of segments given prior to extrapolation, and of presenting the next-but-one position in the series, thereby converting the task into one of interpolation. Where possible, quantitative predictions are made concerning these variables on the basis of certain explicit assumptions.

Results show that accuracy decreases, as predicted, as the gradient of the series increases, but does not seem to be affected by adding to the number of terms presented. Interpolation shows a reduction in variable error compared with extrapolation more or less as required by theory. Finally, discrepancies in the findings are discussed with special reference to their bearing on the validity of the method.

I

INTRODUCTION

The need for a psychophysical method appropriate for investigating a subject's response to certain stimulus sequences, as opposed to isolated stimuli, seems to be indicated by a number of developments in perceptual psychology. Thus Gibson (1950, 1955) on the basis of his studies on the perception of space and motion has often stressed that as distinct from the physiologist what is important for the psychologist are the higher order variables of stimulus sequences or, as he has called it, "ordinal stimulation."

From another direction, communication theorists have familiarized us with the concept of "redundancy" and of its importance in the assimilation of information. However, the redundancy of a prose passage rests entirely upon the subject's familiarity with the conventions of language. Yet Attneave (1954) has shown that a technique can be devised for investigating the redundancy of pictorial information. Here the redundancy is no longer just of a statistical-conventional nature if only because the contours of most objects of our environment exhibit some degree of continuity in a mathematically definable sense, a fact which is presumably exploited in our recognition of objects partially screened from view. Attneave comments that "Any sort of physical invariance whatsoever constitutes a source of redundancy for an organism capable of abstracting the invariance and utilizing it appropriately" and points out that further studies are required to investigate the limits of our perceptual abilities in this respect.

In the field of motor skills, Craik (1948a and b) argued that owing to reaction-time latency certain tracking tasks would not be possible if the human operator depended on continuous feed-back correction and it must therefore be possible for him to exploit any kind of lawfulness in the path of the target to anticipate its course. Gottsdanker (1952, 1954) verified this supposition in a series of experiments involving extrapolation in a motor task. Thus, in one case, the subject was required to continue "tracking" a target moving along a straight line according to some pre-arranged formula *after* it had disappeared from view. In another a subject was required to continue a given "tapping sequence." It is of some interest here to note

that Gottsdanker found that a discrete series such as the latter was more amenable to accurate extrapolation than continuous motion. Such skills as ball games, where the operator intervenes only intermittently, offer perhaps the most striking example of the type of skill which depends on anticipation made possible by the fact that the trajectory of the ball provides an obvious source of redundancy.

The method of serial extrapolation, whose application to one particular type of series is discussed below, is designed to discover something further about this power, which we must assume to exist, of utilizing mathematical lawfulness in stimulus sequences for the purposes of adaptive behaviour. In principle the series used can be expressed in any sensory dimension and be of any desired description provided only that it is "orderly" in the mathematical sense that all its terms can be uniquely specified in a single equation. All that is required technically is that the subject be given some means of indicating what he considers to be the appropriate next term in such a series.

II
The Experiments

In the present experiments the Method of Serial Extrapolation (M.S.E.) has been applied to a visual series in geometric progression obtained by dividing a straight line into segments according to the rule of constant ratio. A second series of hyperbolic type corresponding to the perspective representation of equal distances in depth has likewise been investigated (Beloff, 1956), but this will be discussed in a later paper.

The task here then is for the subject simply to mark off a length of line that should follow on in geometric progression the presented series. This raises immediately two problems fundamental to the application of M.S.E.:

(a) How far does the task lie within the competence of the subject?
(b) How is performance affected by variations in the stimulus series?

The first is simply a question of the basic validity of the Method and calls for an analysis of performance in terms of sensitivity or objectivity. The second involves three variables in particular:

(i)   The rate of increase of the series at the point at which the extrapolation is to be made.
(ii) The number of segments presented in the stimulus series prior to the point of extrapolation.
(iii) The conversion of the task to one of *interpolation* by presenting the next-but-one point after the point of extrapolation.

Three special hypotheses were put forward regarding the effect on accuracy of each of the above variables respectively:

(1) The "P-Hypothesis": (see Notation, Appendix p. 167)

$$\text{Let } \tilde{A}_1 = \text{The extrapolandum segment}$$
$$\text{and } A_0 = \text{The pre-extrapolandum segment}$$
$$\text{and let } P = \tilde{A}_1/A_0 \ [A_1 > A_0]$$

(The clause $[\tilde{A}_1 > A_0]$ has been introduced to make P greater than unity in all cases so that the P-hypothesis should be applicable indifferently both to expanding and diminishing series. Note that for a geometric progression alone the ratio between every pair of adjacent segments will equal P.)

The P-hypothesis then states that

$$\text{V.E.} = f\ (P)$$

which may be expressed verbally as:

"The Variable Error or Scatter (as an inverse measure of accuracy) is a direct linear function of the P-ratio."

(2) The "Overdetermination Hypothesis."

This states that: "Other things being equal, accuracy will be greater where more segments are presented in the stimulus series." The hypothesis is to be tested by comparing performance on Batteries 1a and 1b which present 3 and 4 segments respectively. No quantitative formulation of this hypothesis was possible.

(3) The 'Interpolation Hypothesis.'

This states that: "Accuracy should be greater for Interpolation as against Extrapolation" and is given by the formula

$$\text{V.E.}_{\text{(interp.)}} = \text{V.E.}_{\text{(extrap.)}} \cdot \frac{P}{P + 1}$$

where V.E. = Variable Error, and P is the P-ratio as defined above. The difference between the two tasks should thus appear greatest where $P = 1$, i.e., for bisection as against "duplication" and will decrease asymptotically as P increases.

(For the rationale of these three hypotheses see below under IV. DISCUSSION.)

FIGURE 1

Apparatus.

FIGURE 2

Typical stimulus card.
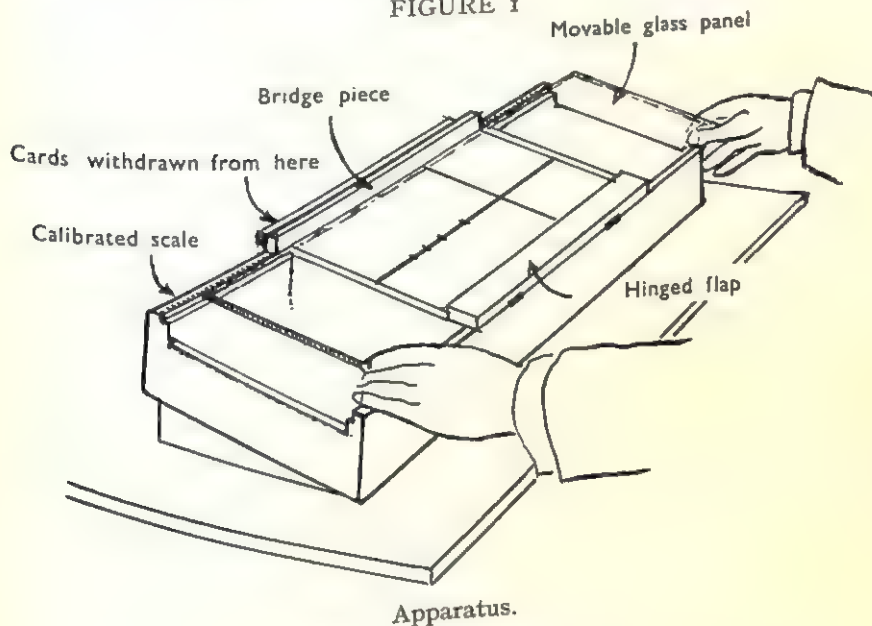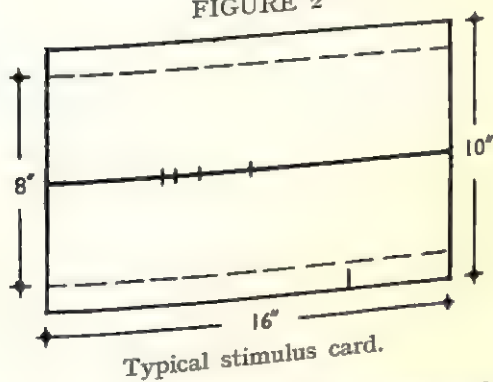
Apparatus

The large number of different stimuli were prepared beforehand on cards and presented to the subject in the appropriate sequence by means of the apparatus (Fig. 1) which was designed to meet the following requirements:

(a) To house the stimulus cards and permit their rapid successive display: a sprung platform allowed the apparatus to be loaded for an experimental session and as each card was removed by the experimenter the next card came automatically into position.

(b) To provide the subject with a variable stimulus with which to make his response: a glass panel was divided vertically into equal parts by a black incised line on its underface.   The whole panel could be moved freely to and fro across the stimulus card so that the vertical marker would cut the horizontal base line of the stimulus series at any point intended by the subject.   Pointers at the ends of the panel enabled the experimenter to make the necessary readings direct from an engineer's scale.

(c) To provide the subject with knowledge of results after each trial: a hinged flap covered the base of each card where a red mark indicated the correct position for the respective stimulus series.   Knowledge of results was thus obtained by simply raising the flap.   A horizontal base line 0·75 mm. wide was drawn across the card with "notches" 3 mm. high and 0·5 mm. wide.   The series was arbitrarily positioned in relation to the card with due allowance for overshooting.   The absolute size of the series was dependent on the pre-extrapolandum segment which was made either 4, 5 or 6 cm.   Only expanding series reading from left to right were used.

Subjects

The 80 subjects used in Experiment I included 45 male and female students doing a first course in psychology and 35 male students all in their third or fourth year of an honours course in engineering.   It was expected that the latter would prove the more accurate judges.   Since, however, the results of Experiment I showed that this was not the case, the 40 new subjects used in Experiment II were drawn from the psychology classes only.

Procedure

For administrative convenience responses were obtained from each subject for each stimulus despite the risk of inter-trial contamination; it was hoped that this latter could be counteracted by a judicious combination of rotation and randomization.

Accordingly, the sequence of presentation was as follows:

All cards of the extrapolation batteries were randomized at the outset and rotated for each successive subject by transferring the top card to the bottom of the set.   For the second half of the population the sequence was reversed.   The interpolation battery was independently randomized and similarly rotated and was presented always after the extrapolation batteries.

In Experiment I nothing was said regarding the nature of the series in question, although immediate knowledge of results was available after each trial.   A few modest monetary prizes were offered for the most accurate scorers.

In Experiment II the nature of the series was explained beforehand but no knowledge of results was available and no incentives were offered for accurate scoring.

It is the peculiarity of M.S.E. that instructions may be either more or less explicit. By not divulging the nature of the series in Experiment I it was hoped that a more natural and intuitive approach could be elicited.   It transpired, however, to the experimenter's surprise, that for the naïve subject the only conceivable type of series is in fact the geometric progression!   Thus a policy of secrecy was either unnecessary or liable to produce a dangerously misleading set.   In the second place it was hoped to eliminate any constant error so that accuracy might be assessed in terms of scatter alone.   Accordingly the conditions of Experiment I were intended to promote maximal objectivity.   The expedients used, however, failed signally to eliminate constant error so that in Experiment II it seemed wiser to allow it to take its natural course by withholding knowledge of results.

Subjects were advised against spending too long on any one trial, but were otherwise left free in respect of the time spent on any trial or the system they might adopt in arriving at a solution.   It was hoped that in this way a clearer understanding of the task might be gained by observation and subsequent questioning.   Each subject had four preliminary practice trials.

TABLE I

MEDIAN RESPONSE RATIOS
$$\overline{p} = \overline{a}_1/A_0$$

EXPERIMENT I (N = 80)

P-ratio	Battery Ia	Battery Ib	Interpolation
I = 1·000	1·056	·	—
√3/2 = 1·225	1·294	1·284	1·229
√φ = 1·272	1·347	1·323	1·300
2 = 1·414	1·352	1·432	1·447
3/2 = 1·500	1·511	1·505	1·545
φ = 1·618	1·516	1·618	1·672
√3 = 1·732	1·602	1·632	—
2 = 2·000	1·805	1·910	—

EXPERIMENT II (N = 50)

P-ratio	Battery Ib	Interpolation
√φ = 1·272	1·371	1·319
√2 = 1·414	1·421	1·447
φ = 1·618	1·561	1·712
2 = 2·000	1·900	2·220
√5 = 2·236	2·091	2·474

TABLE II

VARIABLE ERROR
$$\text{V.E. } \% = Q/\overline{a}_1 \times 100$$

EXPERIMENT I (N = 80)

P-ratio	Battery Ia	Battery Ib	I a & b pooled n = 160	Interpolation
I	4·76	—	5·92	2·12
√3/2	5·14	6·06	5·21	2·08
√φ	5·54	7·12	8·19	3·08
√2	6·02	7·02	6·60	4·22
3/2	7·61	5·68	7·20	4·04
φ	6·23	8·75	7·76	—
√3	10·23	8·38	8·26	—
2	9·56	9·32	—	

EXPERIMENT II (N = 50)

P-ratio	Battery Ib	Interpolation
√φ	6·80	2·86
√2	7·15	3·58
φ	8·03	4·19
2	9·74	4·80
√5	9·76	6·06

III
RESULTS

The evidence on validity

(i) *Objectivity criterion.* In Figures 3–6 median $\bar{p}$ values are plotted against P. Now if $\bar{p}$ were always an objective estimate of P then the plottings should all fall along the diagonal within an understood margin of error. But clearly this is not the

FIGURE 3
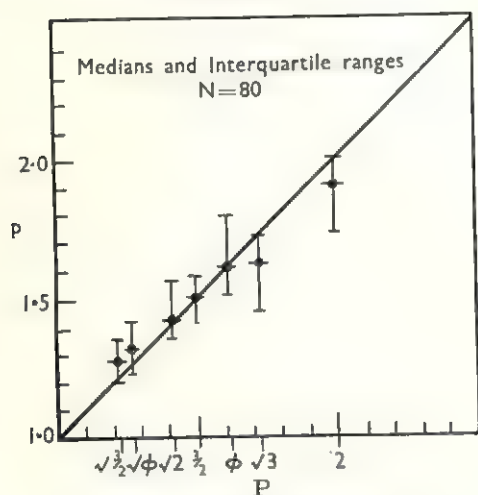
Battery I*b*.   Experiment I

FIGURE 4
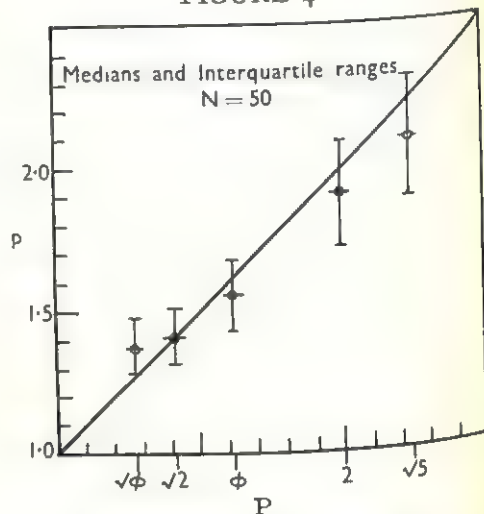
Battery I*b*.   Experiment II.

FIGURE 5
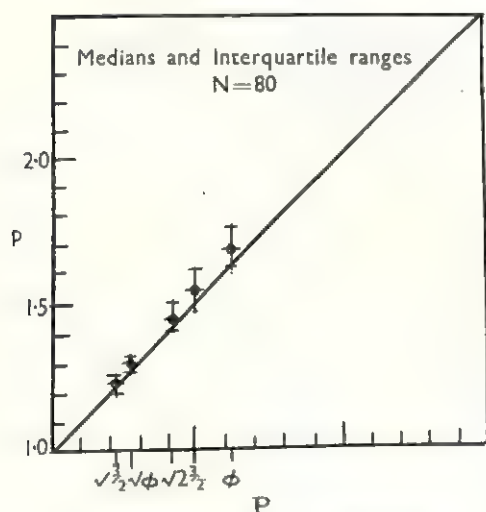
Interpolation battery.   Experiment I.

FIGURE 6
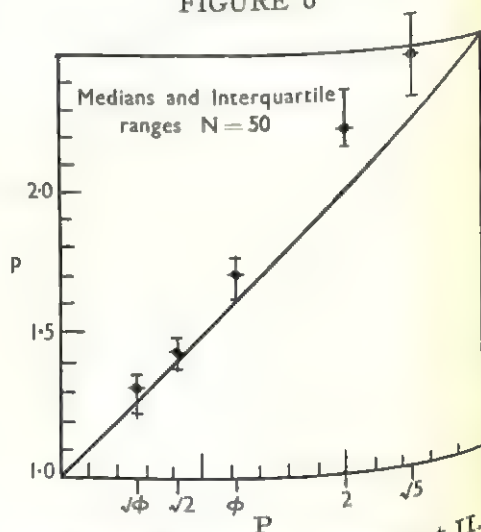
Interpolation battery.   Experiment II.

case. Since the quartiles are plotted as well as the medians it is roughly apparent to inspection whether a particular constant error (indicated by the distance of the median from the diagonal) is significant or not. (It can be ascertained by χ^2 that where the diagonal cuts a distribution at the quartile the constant error is significant beyond the 0·1 per cent. level.) Clearly there are enough constant errors of sufficient magnitude to invalidate belief in the objectivity of judgement among our population.

Examining next the constant error trend we see that for the extrapolation batteries it is at first positive and then becomes increasingly negative as P increases, crossing the diagonal somewhere between $\sqrt{2}$ and ϕ. In the interpolation batteries on the contrary it starts on or near the diagonal and becomes increasingly *positive* with increase in P.

(ii) *Sensitivity criterion.* The differential threshold of a ratio cannot of course be exactly determined from our data. Much more intensive experimentation would be required. Nevertheless, some indication of its value can reasonably be expected. For example, if the differential threshold lay below the interval between any of the neighbouring values of our arbitrarily selected P-ratios, then the *p*-values would constitute a monotonic succession. Now from Figures 3–6 we see that with few exceptions such is indeed the case. A test comparing results on certain neighbouring P-ratios (which has been omitted here for the sake of brevity) suggests that an increase of 10 per cent. on a ratio is safely above the threshold of discrimination in this instance.

(iii) *Learning criterion.* On any performance of a skilled nature evidence of learning might be expected to declare itself. But in our experiments, whether we have regard to progress within a single experimental session or, more tellingly perhaps, to a comparison of results under a condition which allowed knowledge of results (Experiment I) as against one which did not (Experiment II) the evidence is negative both with respect to variable error and constant error.

(iv) *Individual differences.* The existence of such differences may be regarded as characteristics of a true skill and here the evidence is positive and unequivocal.

TABLE III

TEST OF P-HYPOTHESIS

	F	df	Result
Experiment I, extrapolation	20·64	1, 5	Sig. 1% level
Experiment I, interpolation	4·01	1, 3	Non sig.
Experiment II, extrapolation	18·28	1, 3	Sig. 5% level
Experiment II, interpolation	24·41	1, 3	Sig. 5% level

FIGURE 7

Battery 1 (a and b pooled) N = 160
V.E% = 4·39 P + 0·43
V.E% = 4·73 P — 3·55
Interpolation Battery N = 80

Variable error. Experiment I.

FIGURE 8

Battery 1 b N = 50
V.E% = 3·21 P + 2·81
V.E% = 2·86 P — 0·59
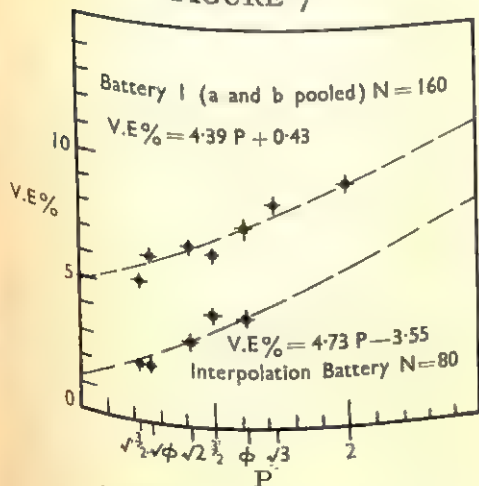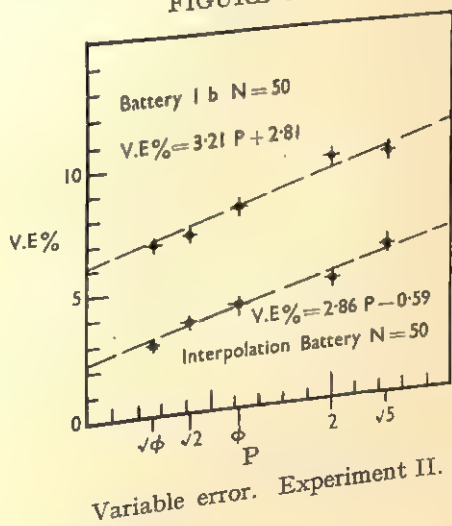Interpolation Battery N = 50

Variable error. Experiment II.

In Experiment I, only, each subject obtained an overall accuracy score being the mean of his deviations on every trial expressed as a percentage of the respective extrapolandum with sign disregarded.   For the 34 original trials given on the geometric series scores ranged from 4·79 per cent. ±0·62 to 16·65 per cent. ±2·11 with median at 8·70 per cent.   Although a full analysis of the individual differences has been omitted these figures give an indication of the wide variation in ability found. In addition a personal bias score was computed for each subject by means of a similar mean percentage deviation but with sign retained.   Here the range was from −5·62 per cent. ±1·54 to +8·79 per cent. ±2·79 with median at +0·60 per cent. There can thus be no doubt as to the existence of systematic overestimators and underestimators.   The fact is perhaps suggestive of an underlying personality distinction.

The P-Hypothesis

Both for extrapolation and interpolation there is a progressive increase in variable error (V.E.) with increase in P in accordance with our P-Hypothesis (Figs. 7 and 8), but it remains to be shown that this increase is both significant and linear.   Unfortunately the plottings are too few to permit a test of linearity, but, if we assume linearity, we may test for significance according to the following analysis of variance (McNemar, 1949, p. 254).

Let y be the observed V.E. values and y' the predicted V.E. values where x is the independent variable and N the number of plottings; then we can establish an F-ratio where

$$F = \frac{\sigma^2_{y'-\bar{y}}}{\sigma^2_{y-y'}} = \frac{(N-2)r^2_{yx}}{(1-r^2_{yx})} \qquad df = 1, (N-2)$$

The null hypothesis is that prediction would be no worse if the regression line were drawn horizontally through $\bar{y}$ (the mean observed V.E. value).   The significances (Table III) are not unequivocal but since five points are few on which to rely for curve fitting especially where the increase is so gradual it may be concluded that the evidence tends to support the P-hypothesis even though further investigation is obviously called for.

The overdetermination hypothesis

No overall improvement in performance (Tables I and II) occurred for Battery Ib as against Ia either in terms of variable error or constant error.   Our hypothesis thus fails to be supported.

The interpolation hypothesis

Figures 7 and 8 represent a comparison between interpolation and extrapolation for each experiment.   It is immediately obvious that there is a substantial reduction in variable error in favour of interpolation.   Now according to our hypothesis this should be expressible by the equation

$$\text{V.E.}_{\text{(interp.)}} = \text{V.E.}_{\text{(extrap.)}} \cdot \frac{P}{P+1}$$

But from the P-hypothesis we also have

$$\text{V.E.} = f(P) \text{ where } f \text{ is a linear function}$$

Let us write therefore

$$\text{V.E.}_{\text{(interp.)}} = k_I P + C_2 \text{ where } k_2 \text{ and } C_I \text{ are constants}$$

and $$\text{V.E.}_{\text{(extrap.)}} = k_E P + C_E \text{ where } k_E \text{ and } C_E \text{ are constants}$$

whence it must follow that

$$\frac{\text{V.E. (interp.)}}{\text{V.E. (extrap.)}} = \frac{k_I\, P + C_I}{k_E\, P + C_E} = \frac{P}{P + 1}$$

But this equation can be satisfied only if we make

$$k_I = k_E, \quad C_E = k, \quad C_I = 0$$

which implies that the regression line for the interpolation values must run parallel to the regression line for the extrapolation values and must, moreover, cut the ordinate at $+k$.

From the regression equations given in Figures 7 and 8 we see that the former condition is approximately fulfilled since

$$k_E = 4\cdot39 \quad k_I = 4\cdot73 \quad \text{(for Experiment I)}$$
$$k_E = 3\cdot21 \quad k_I = 2\cdot86 \quad \text{(for Experiment II)}$$

The latter condition, however, is not fulfilled at least for Experiment I since

$$C_E = +0\cdot43 \quad C_I = -3\cdot55 \quad \text{(for Experiment I)}$$
$$C_E = +2\cdot81 \quad C_I = -0\cdot59 \quad \text{(for Experiment II)}$$

This discrepancy is not as serious as might be supposed except perhaps at the lower end of the P-scale where certain independent trials further showed that "bisection" tended to be *more* than just twice as accurate as "duplication." In conclusion we may state that our formula on the whole underestimated in the present instance the advantages in favour of interpolation.

Summary of results

We may recapitulate our findings as follows:

1. The Method of Serial Extrapolation has been found to be applicable to a series consisting of a line subdivided in geometric progression. There were, however, considerable individual differences and a pronounced and systematic constant error in the combined group results.

2. The evidence suggests that, in accordance with our "P-Hypothesis" the variable error appears to increase in direct proportion with increase in the P-ratio.

3. Accuracy was not improved, contrary to expectation, by adding to the number of terms in the stimulus series beyond the required minimum.

4. A reduction in scatter could be brought about by converting the task into one of interpolation although our formula somewhat underestimated this advantage.

IV

Discussion

Constant error

The persistence of a large and systematic constant error even in the light of knowledge of results was perhaps the most surprising feature of our data and the one which presents the most serious challenge to the validity of M.S.E. The one explanation which seemed to fit was in terms of the "Central Tendency" first adduced by Hollingworth (1909). This phenomenon refers to the tendency of a judgement to be distorted in the direction of the average, or some central value, whenever a subject has to deal with a plurality of stimuli. For our own case the problem is further complicated in as much as it will be necessary to assume two sources of Central Tendency which we shall call respectively C.T.$_1$ and C.T.$_2$. C.T.$_1$ arises from the very nature

of M.S.E. since the use of any series other than an equality series implies the presentation of a range of different stimuli. In the case of an expanding series, which is what we are concerned with here, the extrapolandum segment must be larger than the preceding segments. Hence $C.T._1$ should be negative and should correspond to some small fraction of the difference between the extrapolandum and a central value obtained by the appropriate pooling of all the segments of the stimulus series. $C.T._2$ on the other hand arises solely from the fact that the subject has here been required to perform on several trials, each with a different stimulus series, as a result of which any particular extrapolation should be shifted in the direction of the average extrapolandum for the whole battery. We may conclude that the final response segment a_1 is the resultant of the combined influences of $C.T._1$ and $C.T._2$ on the correct response $\hat{A}_1$

$$a_1 = \hat{A}_1 - (C.T._1 + C.T._2)$$

With a suitable selection of parameters this formula permitted (Beloff, 1956) a reasonable prediction of the constant error trend for extrapolation.

An independent test of this interpretation is possible by running the experiment for a single value of P on a new population with no previous experience of the task. Here $C.T._1$ should alone be operative and so the constant error always negative. Such a test was in fact carried out with $P = \sqrt{3}/2$, a value which produced the largest positive constant error in Experiment I. This now gave a significantly *negative* constant error, thereby serving to support our explanation.

As regards interpolation it must be remembered that there was no negative constant error but only a steadily increasing positive constant error. Can the same set of assumptions cover this case as well? A priori it may seem as if no constant error should arise since a shift in a_1 in either direction must produce a reverse shift in a_2. The fact, however, that a positive constant error does occur making $a_1 > \hat{A}_1$ and $a_2 < \hat{A}_2$ makes it appear that the central tendency is governed by the initially undivided segment $X_1 X_3$ (Fig. 9). This would give us the positive value we require for $C.T._1$. What of $C.T._2$? To arrive at a satisfactory formula we found it necessary to assume that $C.T._2$ is negligible. It is after all not implausible that with the greater accuracy possible with interpolation we should get a lesser degree of inter-trial contamination.

But if a large constant error is indeed inseparable from its application can M.S.E. still be considered a valid psychophysical procedure? To answer this we must remember that although a constant error emerges from the pooling of group results that effect is not universal. As we have seen there are even subjects who consistently overextrapolate. The evidence suggests that, if one were to use only the more skilled subjects or else introduce sufficient preliminary training, central tendency might well be virtually eliminated. For an analysis of individual differences in relation to central tendency see Lambercier (1946).

The P-hypothesis

Let us consider first the rationale of this hypothesis: where P is unity, then for any type of series we have the limiting case of an equality series in which extrapolation becomes equivalent to simple reproduction. Now if we accept that a judgement of equality represents psychologically the simplest instance of a ratio judgement, then we must expect that where P is unity accuracy will be at a maximum. It does not follow from this, however, that, as postulated by our P-Hypothesis, accuracy declines in direct proportion as P increases. It might be that accuracy falls away steeply as P exceeds unity but thereafter remains stationary, or it might be that

certain special ratios, perhaps those with integral values, are inherently easier to judge than others.

The argument in favour of our present formulation of the P-Hypothesis, which has at least the merit of simplicity (for in effect it merely extends the Weber-Fechner law to ratio-magnitudes), is based on the assumption that the error involved in a ratio judgement is compounded of the separate errors involved in judging its component terms as set out in the Appendix, p. 167.

However, a test of the alternative hypothesis that there might be special preferential ratios was allowed for by a suitable selection of P-ratios. We have already mentioned in this connection those of integral value and although we have not here been concerned with integers above two, geometric progressions can be constructed in which *alternate* terms stand in integral ratio to one another by making P the square root of an integer (e.g., $1 : \sqrt{3} : 3 : \sqrt{3 \times 3}$: etc.). Moreover, it so happens that proportions formed by taking the square root of the first three prime numbers ($\sqrt{2}$, $\sqrt{3}$, $\sqrt{5}$) have traditionally been accorded a privileged status in the aesthetics of Design (Ghyka, 1946). From the aesthetic point of view, however, the supreme ratio has always been the so-called "Golden Section" or ϕ-ratio which obtains when the lesser part stands to the greater as the greater to the whole ($1 : \phi : : \phi : 1 + \phi$) giving as the value of ϕ, $\dfrac{\sqrt{5} + 1}{2}$ or 1·618 approximately. Since by definition each term of a ϕ-series must equal the sum of the two preceding terms there is further reason for suspecting that the ϕ-ratio might facilitate judgement, aesthetic considerations apart.

Accordingly, instead of taking our P-ratios at equal intervals along the number continuum, we chose certain of these hypothetically privileged ratios so that they might be compared with neutral ratios such as 3/2. The results, however, failed to provide any evidence in favour of this theory of special status ratios, though in view of the crudity of judgement and pronounced constant error evident in our data we cannot conclude that this has disposed of the theory. There was, it is true, some slight evidence that for the case of P = 1 the variable error was somewhat less than would be predicted from the regression lines based on all other ratios, so that should further evidence confirm this we should have to say that unity, at least, constitutes a special status ratio in the psychology of perceptual judgement.

The overdetermination hypothesis

Here the rationale is not immediately apparent as in the case of the Redundancy Hypothesis of the Information Theorist. In extrapolating the next letter of an unfinished sentence the range of possible choices is clearly reduced as more of the verbal context is supplied. In a mathematical series, on the other hand, once the governing equation is given then a certain number of terms only (two in the case of a geometric progression) is sufficient to determine uniquely all other members of the series. If, however, the governing equation is not known, the instructions being merely to preserve an orderly series, then in theory the range of choice is infinite, even if in practice a preference for the simplest orderly series which the circumstances permit may be expected. Hence it might be argued that for a mathematical series nothing equivalent to a Redundancy Hypothesis is applicable. Nevertheless, if we consider the psychological process of extrapolation some such hypothesis does seem to be in order.

First, where any single judgement is subject to random fluctuation the pooling of a set of judgements should on statistical grounds reduce the magnitude of the

error. The presentation of a succession of ratios might in the present instance induce such a pooling process.

Secondly, verbal instructions, however explicit, cannot make the task comparable to the programming of a computor. The human subject seems to operate in an impressionistic manner and one has to think rather in terms of somehow conveying to the subject the proper rhythm of development for the series in question.

Lastly, there is empirical evidence from a different quarter which has some bearing on our hypothesis. In a study on judgement of direction (Salomon, 1947) the subject was required to position a point at a fixed distance from the end of a segment of a straight line so that it should lie on the prolongation of that line. It was found that the length of segment presented *was* effective in reducing error. However, all attempts so far to confirm this hypothesis for our case have failed including those experiments (Beloff, 1956) in which the series was overdetermined by being presented two-dimensionally in the form of rectangles, circles, spirals, etc. Before, however, relinquishing an hypothesis for which so many suggestive parallels can be found the shortcomings of our present set-up should be carefully scrutinized. Thus, in the first place since the general level of accuracy was not high the subjects may simply have lacked the skill to utilize the additional information. Secondly, since the variable stimulus was directly manipulated by the subject the stimulus could not be more than an arm's length from his eye. This meant that in certain cases the extrapolandum plus the pre-extrapolandum segments might subtend an angle of as much as 15°. Consequently, though the subject might be free to scan the entire series he would inevitably tend to concentrate on the segments closest to the extra-plandum on which he would be fixated and so ignore segments added to the other end of the series. Introspective reports suggested that such was indeed the case. Adjust-ment by remote control would have permitted the entire series to be conveniently subtended in foveal vision.

The interpolation hypothesis

That interpolation should prove easier than extrapolation seemed obvious for various reasons. The problem was to find a formula which would predict the exact extent of this difference. The formula we have adopted, the proof of which has here been omitted for reasons of space, was arrived at by starting with the simplest possible assumption, namely that there is no *intrinsic psychological* difference between the two tasks. It can then be shown that the difference given by our formula should nevertheless arise in the respective response distributions. This is due to the fact that a different error term becomes operative for the case of interpolation and would disappear if a different scoring system were adopted to compute the variable error.

Such discrepancies as emerged from the results were not large and it is suggested that they can be easily accounted for by the failure of certain assumptions to hold for the present set-up. In particular we ignored the possibility of constant error which, since it was positive for interpolation but for the most part negative for extra-polation, invalidated to some extent any straightforward comparison. Further, we assumed small random fluctuations but, especially in the extrapolation trials, there was a fair amount of freak overshooting. To try and allow for such contingencies in the formula itself would be to introduce excessive complication but a test of our original hypothesis could still be made using a selected population of specially accurate subjects.

V

APPENDIX

1. *Notation and Scoring System:* (see Fig. 3)

In our notation capital letters stand for stimulus variables and lower case letters for response variables. A circumflex denotes correct values and a bar median response values. The following symbols are used:

X_2 = The extrapolandum point $\qquad$ x = An extrapolated position
$\hat{A}_1$ = The extrapolandum segment $\qquad$ $\bar{a}_1$ = The median extrapolated segment
P = $\hat{A}_1/A_0$ = The extrapolandum ratio $\qquad$ $\bar{p}$ = $\bar{a}_1/A_0$ = The median extrapolated ratio

FIGURE 9

Extrapolation:

Interpolation:
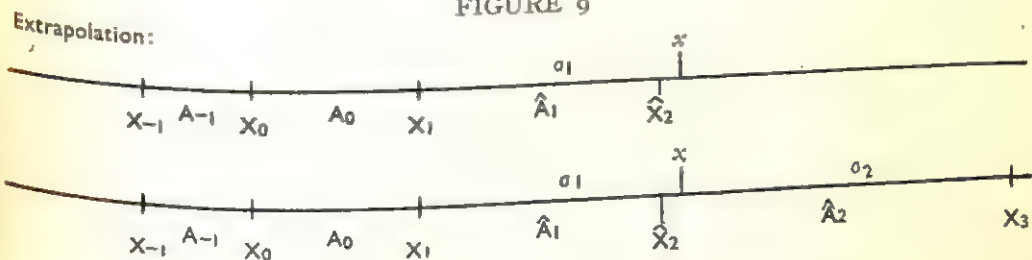
Notation.

We are here concerned with two independent measures of accuracy: the Constant Error (C.E.) and Scatter or Variable Error (V.E.).

C.E. % = Constant Error as percentage of $\hat{A}_1 = \bar{a}_1/\hat{A}_1 \times 100$ (= $\bar{p}/P \times 100$)
V.E. % = Semi-interquartile range as percentage of $\bar{a}_1 = Q/\bar{a}_1 \times 100$.

Note that on account of the prevalence of a significant constant error V.E. % has been taken as the index of variable error rather than using simply Q %.

2. *The P-Hypothesis*

To prove: V.E. = f (P)
where V.E. = Variable Error, f = some linear function, P = A_1/A_0

We may start by assuming that the error involved in a ratio judgement is compounded of the separate errors involved in judging its component terms. We need next a postulate at the perceptual level analogous to Hull's "Behavioural Oscillation." It is suggested that any given physical stimulus A corresponds to a range of possible psychological stimuli A $\pm$ δ, where δ is some small margin of perceptual ambiguity. Note that δ should not be confused with the familiar differential threshold ΔA, for whatever the psychophysical method employed, the differential threshold presupposes a comparison between two stimuli, a standard and a variable. Hence it may be conceived as merely a special case of a ratio judgement with P equal to unity whereas δ is ex-hypothesi, the ambiguity of a single stimulus without reference to a second. This means incidentally that δ is purely a hypothetical entity which cannot be directly measured.

Now by definition P = A_1/A_0 so $p = \dfrac{A_1 \pm \delta_1}{A_0 \pm \delta_0}$ there will thus be two equiprobable limits for p, viz.: $\qquad$ .. .. .. (1)

p min. = $\dfrac{A_1 - \delta_1}{A_0 + \delta_0}$ and p max. = $\dfrac{A_1 + \delta_1}{A_0 - \delta_0}$ .. .. .. Now

Let us assume that δ is always some small constant fraction of A such that $\delta = kA$. Now let $\pm \Delta P$ be the differential threshold of a ratio and let us write: .. .. (2)

then $\qquad \Delta P = \dfrac{p \text{ max.} - p \text{ min.}}{2}$ .. .. (3)

$$2\Delta P = \frac{A_1}{A_0} \left(\frac{k + 1}{1 - k} - \frac{1 - k}{1 + k} \right) \quad ..$$

$$= \frac{A_1}{A_0} \frac{(k+1)^2 - (1-k)^2}{1-k^2} = \frac{4A_1 k}{A_0 (1-k^2)} \qquad \qquad \qquad \qquad (4)$$

$$\therefore \Delta P = \frac{2A_1 k}{A_0 (1-k^2)} \qquad \qquad \qquad \qquad \qquad \qquad \qquad (5)$$

but ex-hypothesi k is some small fraction, hence $1 - k^2 \simeq 1$, also $\dfrac{A_1}{A_0} = P$

$$\therefore \Delta P = 2kP \text{ or } \frac{\Delta P}{P} = C \text{ (where } C = 2k) \qquad \qquad \qquad (6)$$

(where $P = 1$ we have $A_1 = A_0 = 1$, $\Delta P = \Delta A = 2k = 2\delta$)

but since the Variable Error is a function of discriminability and thus of the size of the differential threshold we can also write

$$\text{V.E.} = f(P)$$

I wish to thank Dr. George Seth for reading through the manuscript and suggesting many valuable improvements and Mr. John Whitfield for constructive criticism of my Ph.D. thesis from which this material is drawn.

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THE DEVELOPMENTAL STUDY OF APPARENT MOVEMENT

BY

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Differences in thresholds of apparent simultaneity and apparent movement were observed with normal and brain-damaged children of different ages. In an attempt to explore this relationship more fully, 111 subjects between 2½ years and 19 years were tested on a modified Withers tachistoscope. It was found that changes in thresholds of apparent simultaneity and apparent movement were clearly a function of age. Both illusions were seen under objectively more difficult conditions—slower flash frequencies— by younger children. The decrease of the illusions with age continued until about 14 years, and then levelled off. No sex differences were observed. A second investigation of closure ability indicated that an explanation of the apparent movement changes in terms of greater closure ability in younger children was not tenable, since ability to form a gestalt under difficult conditions increased as a function of age. In the light of other investigations on age and illusions it is possible that the change in apparent movement is due to an increase of discrimination with age. On the other hand, the similarity between the frequencio of apparent-movement thresholds and dominant brain rhythms, as well as their parallel changes with age, may point to some more basic neurophysiological growth change.

I

INTRODUCTION

In an investigation of apparent movement in brain-damaged and normal children, the author observed that individual differences in threshold appeared to be related to the age of the subject. In ascending trials, i.e. from apparent simultaneity to succession, younger children reported movement later (at slower flash frequencies) than older children; similarly they reported succession later, or not at all. In descending trials, i.e. from succession to apparent simultaneity, younger children reported apparent movement at flash frequencies at which older children and adults report succession, and apparent simultaneity at frequencies which generally evoke the perception of movement in older children and adults. Moreover the mean thresholds of the children's group was somewhat lower than that obtained previously by the author in a large group of adults. A similar finding has been reported by Meili and Tobler (1931) who compared 38 children from 5 to 12 years with 22 adults. They found that both simultaneity and apparent movement appeared more readily in the children's groups. It seemed worthwhile to repeat Meili and Tobler's study using a larger group of subjects to investigate, not merely the difference between children and adults, but more particularly the range of thresholds within the children's group, and to see whether this range was a function of age. Therefore 111 subjects from 2½ to 19 years were tested on apparent movement in an attempt to see whether the changes in threshold with age followed a growth curve.

EXPERIMENTAL

Apparatus

The apparatus consisted of a modified Withers tachistoscope (Withers 1954) built in the Central Workshops of the Council for Scientific and Industrial Research. The figure was placed in front of neon lights in a dark viewing box 20 inches from the subject. This The stimulus figure was an incomplete, inverted "vee" formed by an angle of 35°.

figure evokes a pendulum-like movement. The exposure time for each light was 25 ms., and the frequency of the light flashes ranged from 16·67 f.p.s. to 1·11 f.p.s. (time intervals ranging from 25 ms. to 875 ms.) in 25 graduated steps. The timing device was a completely silent electronic timer, and the frequency changes were regulated by a silent motor driven at constant speed.

Procedure

Each subject looked into the viewing box and was shown the stimulus figure at 16·67 f.p.s. (apparent simultaneity) and asked to say what he saw. As soon as the report of two simultaneously visible lines was given, a magnetic clutch released the motor so that the stimulus lights flashed at progressively slower frequencies. The subject was asked to report any change. When a clear report of pendular movement was obtained, the subject was asked to report any further change. If no spontaneous report of succession was given on the first two trials (1 ascending and 1 descending) the subject was asked to watch carefully to see whether the line stopped moving so that he could see it first on one side and then on the other, or not. Each subject was given 4 trials, 2 ascending and 2 descending.

Subjects

The subjects were 111 normal children from different socio-economic backgrounds, and varying considerably in scholastic achievement as well as in their approach to the testing situation. The number of subjects in each age group, and their sex, is given in Table I.

TABLE I

AGE AND SEX OF 111 SUBJECTS

Age	M	F	Total
$2\frac{1}{2}$–$3\frac{1}{4}$	3	0	3
$3\frac{1}{2}$–$4\frac{1}{4}$	3	6	9
$4\frac{1}{2}$–$5\frac{1}{4}$	4	4	8
$5\frac{1}{2}$–$6\frac{1}{4}$	5	3	8
$6\frac{1}{2}$–$7\frac{1}{4}$	5	5	10
$7\frac{1}{2}$–$8\frac{1}{4}$	5	3	8
$8\frac{1}{2}$–$9\frac{1}{4}$	6	4	10
$9\frac{1}{2}$–$10\frac{1}{4}$	6	3	9
$10\frac{1}{2}$–$11\frac{1}{4}$	8	1	9
$11\frac{1}{2}$–$12\frac{1}{4}$	2	5	7
$12\frac{1}{2}$–$13\frac{1}{4}$	2	5	7
$13\frac{1}{2}$–$14\frac{1}{4}$	5	3	8
$14\frac{1}{2}$–$15\frac{3}{4}$	3	4	7
16 –19	2	6	8
Total ..	59	52	111

RESULTS

Several of the 3-year-old subjects refused to co-operate, but all other subjects spontaneously reported simultaneity and movement on the first trial. Many subjects did not distinguish succession. The mean lower and upper thresholds are shown in Figure 1.

It can be seen that there is a gradual increase in the flash frequency optimal for the perception of both apparent simultaneity and apparent movement with age. This means that both apparent simultaneity and apparent movement are perceived at slower frequencies by younger children, i.e. children report both simultaneity and movement, under objectively more difficult conditions. This confirms the results found by Meili and Tobler (1931). Moreover the decrease of the illusion with age clearly follows a growth curve, from 3 years to approximately 14 years.

FIGURE 1
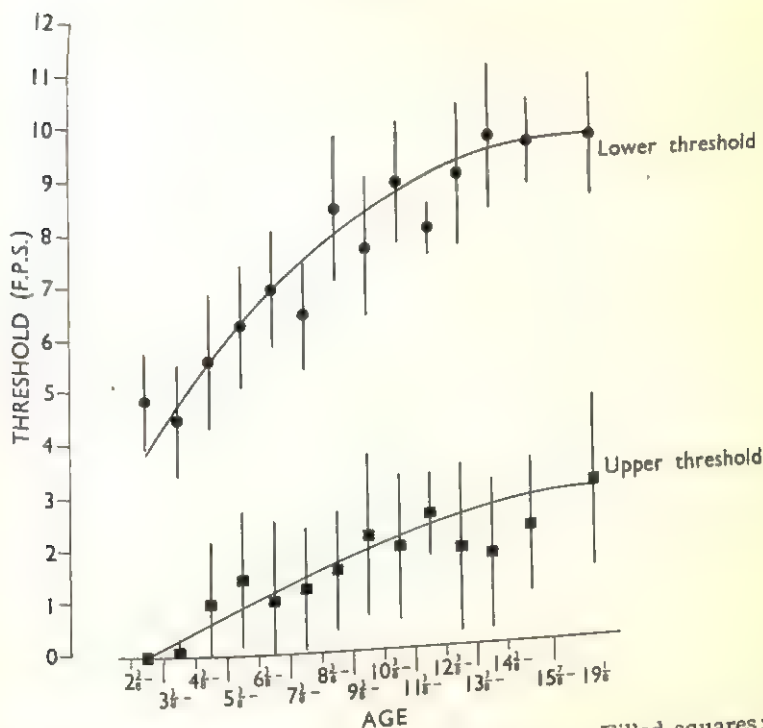
Changes in thresholds of apparent movement with age. Filled squares: mean upper thresholds for each age group. Filled circles: mean lower thresholds for each age group. The bars represent one standard deviation on either side of the mean.

Like Meili and Tobler (1931) the present author found that the children give clear and spontaneous reports and are not influenced by suggestions made by the experimenter. However *lower* thresholds (from simultaneity to movement) of young children are influenced by ascending and descending order of trials—a result not observed in older children or adults. The influence of the order of presentation on *upper* thresholds (from movement to succession) is well known. In ascending from movement to succession, movement is perceived at slower flash frequencies than it is when the order of presentation is from succession to movement. This observation was noted recently by the present author (Brenner 1956) and reported more fully by Jeeves and Bruner (1956).

The variability, expressed in terms of s. d. and shown in Figure 1, remains fairly constant for both upper and lower thresholds. The low variability in the upper threshold in the first two groups is due to the fact that only one child distinguished succession. The reason for the relative homogeneity of group 11½–12¼ is not known, and is probably due to chance.

Contrary to expectation the mean thresholds for boys and girls of each age group are very similar, and there is little evidence that the earlier physical maturation of girls is reflected in apparent movement thresholds. Girls tend to have slightly higher thresholds than boys from 12¼ years to 15¾ years, but these differences are not significant.

In two families with 4 siblings in each, there was no overlap in the lower threshold —i.e. the younger the child the longer he retained the illusion of apparent simultaneity. Moreover, in two cases where the child was known to be socially and

emotionally retarded, his change from simultaneity to movement was reported at flash frequencies considerably slower than the mean for his group. The reverse *change at relatively faster frequencies* for children more socially mature was not observed.

II

INTRODUCTION

Meili and Tobler (1931) considered it probable that sensation lasted longer in children than in adults, and that such longer duration of sensation might explain the perception of apparent simultaneity and apparent movement at longer time intervals. To test this hypothesis they measured the c.f.f. of their subjects on a Talbot fusion wheel, but found no difference between the two groups. They therefore suggested a second hypothesis: that both apparent simultaneity and apparent movement were gestalt processes, and that children had greater facility in forming a gestalt. That strength and speed of closure might be related to the ability to see apparent movement has also been suggested by Biesheuvel (personal communication). Biesheuvel suggested that the central factor of speed of closure (Thurstone 1944), might in fact be proved measurable by apparent movement perception. From the results of the apparent movement investigation it was therefore predicted that younger children would form gestalts more readily than older children. It was considered that this would be found at least within the school going age range of approximately 5 years to 16 years. The material chosen represented pictures within the range of normal experience of young children, e.g. dogs, horses, an engine, a telephone, and several human figures. To test this hypothesis, the same children were tested on the Mooney Closure Test, a test similar to the Street Completion Test, which measures speed of closure of a relatively difficult gestalt. (Mooney 1954.)

EXPERIMENTAL

Apparatus

Ten Cards from the original Mooney Closure Test were chosen on the basis of being the cards most frequently seen by a group of 200 students. These were presented in increasing order of difficulty.

Procedure

Subjects were tested individually. Each subject was told that the cards were pictures of real objects which might be difficult to see at first. He was instructed to keep looking at the card and say what picture it was. If a false response was given, the experimenter suggested he try again and see if he found something different. Each card was removed after 30 seconds. Two practice trials were given first.

Subjects

The subjects were all those tested on apparent movement.

RESULTS

The mean number of correct responses as a function of age is shown in Figure 2.

It can be seen that ability to achieve closure, at least on this test, increases sharply and steadily with age to approximately 14 years. The correlation between the Mooney Closure Test scores and the lower threshold of apparent movement, measured in flashes per second, is $r = 0.66$, which is significant at the 1 per cent. level. This means that the ability to maintain the illusions of apparent simultaneity and apparent movement at lower frequencies is related to the inability to achieve closure on the Mooney Test. It follows, therefore, that Meili and Tobler's explanation of their results in terms of greater closure facility in children (Meili & Tobler 1931) is not tenable.

FIGURE 2
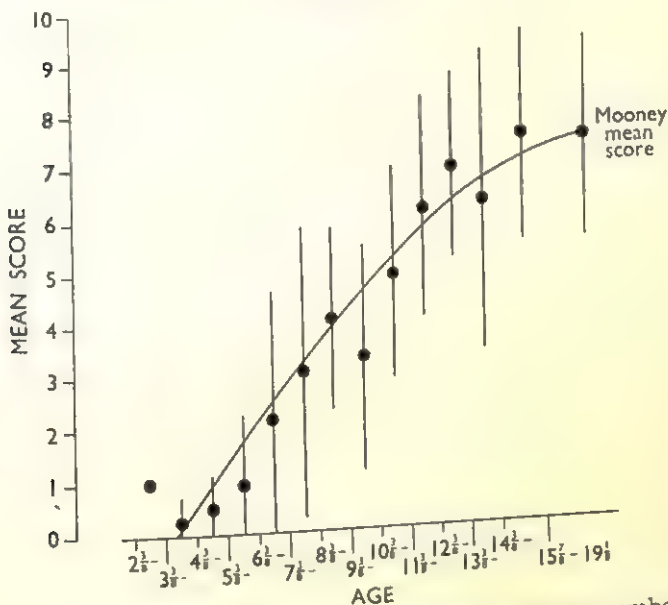
Changes in closure with age.   The circles represent the mean number of correct responses on ten Mooney Test cards for each age group.   The bars represent one standard deviation on either side of the mean.

DISCUSSION

The decrease of the illusions of simultaneity and movement with age can be considered in relation to the decrease of some other illusions as a function of age. Walters (1942) found that both the Müller-Lyer and the vertical-horizontal illusions decreased with age, up to 19 years, although this trend was interrupted at puberty. Recently Fraisse and Vautrey (1956) found a similar decrease of the vertical-horizontal illusion with increasing age, but the amount of change was modified by the type of figure and the length of time it was exposed.   The explanation for the decrease of such illusions with age is not really known.   Certainly the change in apparent movement thresholds is not due to greater facility for closure in younger children, nor is it related to intelligence, subject's attitude nor to specific experience with particular object movements.   (Brenner 1953, 1956.)   It is possible that discriminative ability in general increases with age and experience, and this may explain the changes in apparent movement, Müller-Lyer, and vertical-horizontal illusions with age.   It is difficult to understand why an increase in discriminative ability should be reflected in increasing closure ability, and it is possible that these two variables are quite unrelated.   Closure ability of representational material may be affected by education and experience, although neither of these factors appear to be operative where age is kept constant.   In a homogeneous group of over 200 second-year medical students, the present author found a range of 0 to 90 per cent. of Mooney Test pictures were correctly seen.   All that does emerge clearly from the present investigation is that strength and speed of closure cannot explain facility of apparent movement perception in children as suggested by Meili and Tobler (1931).

In considering apparent movement perception only, it is interesting to note that both the mean flash frequency as well as its modification as a function of age is not unlike the frequency of the dominant brain rhythms found in the EEG's of normal

children (Walter 1950).   Further work on the relationship between such rhythms and apparent movement is now in progress.

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MISCELLANEA

NOTE ON AN ATMOSPHERE EFFECT IN ADULT REASONING

BY

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Reasoning may be influenced by the global impression of the problem, certain problems predisposing the thinker to accept that unique solution which "looks right" although it may or may not be correct. Woodworth & Sells (1935) have called this phenomenon the "atmosphere effect." These writers were not, of course, the first to recognize the existence of this effect. But they do seem to have been the first to demonstrate, by experimental as opposed to anecdotal evidence, that the effect may derive not merely from some arbitrary wording of the problem but also from the problem's basic formal structure. And they showed the operation of such an effect in the syllogistic reasoning of adults. In a recent study of the reasoning of 11- and 16-year-olds, Hunter (1957) obtained clear evidence of an atmosphere effect deriving from the structure of simple series problems (along with a decreasing susceptibility to this effect with increasing age). The purpose of this note is to report a further case of atmosphere effect which derives from structural characteristics of the problem. The problem concerned came to the writer's notice during a class experiment designed to verify Korte's laws of apparent visual movement. Having verified the first two laws, the writer suggested that the third law could be deduced from the other two and was surprised to find both his students and himself arriving at the wrong conclusion. Several of the writer's colleagues were told of this incident, and they too fell victims to the particularly compelling atmosphere of the problem. So it was decided to verify these casual observations by presenting the "bare bones" of the problem to a large number of subjects under relatively standard conditions.

The problem was presented to three different assembled groups of students taking First Ordinary Psychology at Edinburgh University. The total number of students taking part was 120. When each student had been supplied with a blank sheet of paper, the problem was read out to the group as follows:

"In an experiment, it was found that three conditions were inter-related. Call these conditions, A, B, and C, respectively. When A was held constant, the value of B increased with an increase in the value of C. That is, B was directly proportional to C. This may be written as. . . ." The statement A).B $\propto$ C. was written on the blackboard. "Also when B was held constant, the value of C was directly proportional to the value of A. This may be written as . . . ." Directly beneath the first statement was written B).C $\propto$ A. "Now the problem is this. When C is held constant, what is the relationship between the values of A and B?" The statement C).A ?B. was written up beneath the other two statements. "Let me go over that again. There are three inter-related variables, A, B, and C. When A is constant, B is directly proportional to C. That is, B increases when C increases and decreases when C decreases. When B is constant, C is directly proportional to A. And the problem is: when C is constant, what is the relationship between A and B? You have five minutes in which to work out the answer."

When the five minutes were up, the subjects were asked to stop, underline their answer to distinguish it from any rough working, and answer two further questions. First: "If you only had time enough to listen to the problem but not to reason it out, what would you guess the answer to be? In other words, what would be your first impression of the answer?" Secondly: "Did you reason out your answer to the problem? Or did you know the answer already from your studies of, for example, physics, mathematics, or logic?"

When the papers were collected, each subject was allocated to one of five categories according to the answer given. These answers were: I. A is directly proportional to B; II. A is inversely proportional to B; III. No answer given; IV. A is equal to B; and V. A and B are constants. The number of subjects falling into each of these five categories is shown in Table I. Corresponding to these five answers to the problem

were five answers concerning "first impression." Each row of Table I shows the number of subjects who, having given one particular answer to the problem, then reported this or that first impression. The last column shows the number of subjects in each category who reported having known the problem's solution prior to the experiment.

TABLE I

Answer	*No. Ss.*	*"First impression"*					*Answer known*
		I	*II*	*III*	*IV*	*V*	
I	75	63	1	8	3	0	0
II	19	9	8	2	0	0	6
III	15	6	0	9	0	0	0
IV	7	5	0	0	2	0	0
V	4	3	0	0	0	1	0
Total	120	86	9	19	5	1	6

Table I requires little comment. It shows (to the nearest whole number): that 72 per cent. of subjects saw the problem as suggesting answer I; that 63 per cent. arrived at this answer as their conclusion; and that only 11 per cent. arrived, by reasoning rather than by previous knowledge, at the correct solution. Of the subjects giving answer I, 11 remarked that they had arrived at this answer more by guess work than by reasoning, but the great majority actually stated that they had reasoned out their answer, and most of these left some rough working on their papers. There can, then, be no doubt that a strong atmosphere effect operated in the attempted solving of the problem presented.

In order to solve the problem, it is necessary for the thinker to reason somewhat as follows. C increases if A increases, and C also increases if B increases; therefore, if C is to be constant, A and B cannot increase simultaneously; therefore, the one must decrease as the other increases. That such reasoning is difficult for even educated adults to achieve is evident from the results. Yet the relations among the three terms are not unfamiliar and the A, B, and C of the problem might have been replaced: by the intensity, time difference, and distance variables of Korte's laws; by the conductor resistance, current strength, and electro-motive force of Ohm's law; by the temperature, pressure, and volume of Boyle's law; by the length, height, and area of a rectangle; or by the height, cross-sectional area, and volume of a three-dimensional solid or container. This last suggested version of the problem raises the point that the difficulty found by the students at an abstract, symbolic level of functioning is directly comparable to the difficulty (at a more concrete, perceptual level) found by children up to about 6 years who have not yet attained "a concept of conservation of quantity" (see, e.g. Piaget, 1952, chap. 1). The present experiment can thus be taken as further demonstrating two familiar but nonetheless puzzling phenomena. First, the operation of a structurally derived atmosphere effect in adult reasoning. Secondly, the recurrence, at a higher level of reasoning, of a difficulty formally identical to one which has already been met at a lower level of reasoning and overcome at an earlier stage of development.

The writer thanks Dr. Margaret C. Donaldson for drawing his attention to the relation between the present findings and those of Piaget.

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PERIODICITY OF RESPONSE IN OPERANT EXTINCTION

BY

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The distribution of responses during experimental extinction trials for a group of 11 rats, trained in the Skinner-box, was examined. Our analysis supports an earlier suggestion that the extinction trial may be separated into *respondent* and *silent* periods. Response rates during respondent periods show relatively little decline throughout the extinction period investigated, whereas silent periods increase with positive acceleration. The traditional exponential function used to describe response rates during experimental extinction trials should therefore be regarded as a very rough approximation to the mathematical characteristics of the animal's behaviour. Its use as a basis for quantifying critical intervening variables, such as *habit strength* or *behaviour potentiality*, is consequently seriously challenged.

I

INTRODUCTION

A hungry rat can learn to press a lever to obtain a small quantity of food. The resultant strength of this habit may be ascertained in a number of ways; the most common method involves a count of the number of responses made during an extended period when food is withheld (experimental extinction). In the classical studies of Perin (1942) and Williams (1938) the total number of responses prior to a 3-minute interval between two successive responses was used to assess "behaviour strength," whereas Youtz employed a 20-minute inter-response interval for the same purpose. Skinner (1938) amongst others has reasoned that the rate of change in responding, i.e. $\frac{d^2n}{dt^2}$, where "n" refers to the number of responses and "t" to time, should yield a more sensitive measure than the cruder methods of Perin, Williams and Youtz.

It will be seen that these three measures are intimately related to each other. Thus, in order to determine the rate of change we require a set of values for the number of responses which have occurred within a set of t-values; also, given the rate of change of responding, the number of responses likely to occur prior to a response interval, t, could be estimated. Most writers, e.g. Hull (1943) have assumed that the number of responses as a function of time during experimental extinction trials may be expressed by a negative exponential equation of the form .. .. (1)

$$r = R(1 - e^{-bt})$$

where r is the number of responses, R the asymptote, sometimes called the reflex reserve, t the time from the first response of the series and b is a constant; i.e. the *rate*

of response is proportional to the number of responses yet to be made ($R = r$), since

equation (1) is a solution of the differential equation $\dfrac{dr}{dt} = b(R - r)$. This equation

satisfies the requirement of proportionality between rate of response and the remaining number of responses to be made. The more general form of this equation is, of course,

$$Y = c - ae^{-bx} \quad .. \quad .. \quad .. \quad .. \tag{2}$$

where c and a are assumed to be equal. To solve this equation, given that $c = a$, the values of c, b and X must be stated. There is no rational method of checking constants c and b. Consequently c must be empirically obtained to give the best fitting curve for the experimental data. Where c, for reasons of experimental design, cannot be obtained, limited use may still be made of the experimental data, given the supposition that it is exponentially distributed. For example, the differential effect of a given independent variable on the performance of a group of animals trained under identical conditions and therefore reaching the same degree of competence in the performance of the task, may be assessed by a critical ratio or t-test *provided* c_1 and c_{11} can be assumed to be equal. As is well known, this procedure is commonly adopted to evaluate the outcome of an experiment without further attempts to establish the equality of the constants c for the two populations.

The assumption that inter-response intervals during operant extinction are exponentially distributed has far reaching theoretical implications. Hull (1943), for instance, maintains that it reflects the mathematical character of habit growth and weakening and for this reason is of primary interest to the study of learning. Skinner's (1938, 1955) position is clearly defined by his claim that "rate of responding" constitutes the basic datum for the analysis of behaviour, and of learning, in particular. It is therefore surprising that no account is thus far available relating to the actual distribution of inter-response intervals in a typical bar-pressing situation during experimental extinction trials.

A first step towards remedying this situation consisted in ascertaining the duration of the lever-holding responses (Hurwitz, 1954). It was argued that a separation must be effected between the duration of the response and the inter-response interval. We were able to show that if the interval between successive response commencements is used as an approximation to the inter-response interval, a systematic error enters the scores due to the progressively increasing response-durations. Furthermore, it was found that the animal tended to divide its time between bursts of intensive lever-pressing and increasingly longer periods to washing, sleeping, explorations and efforts to escape from the apparatus. Consequently we defined two separate measures: r_{BG} was defined as the rate of response within a response-burst, or an *active* period; r_0 was defined as the rate of successive response-burst occurrences. Clearly the division of the experimental session into *active* and *silent* periods questions the traditional procedure of describing free-operant extinction in terms of a *single monotonic function*. We have therefore adopted the following experimental procedure to shed more light on our earlier contention: (*a*) the extinction record of 11 animals was examined in order to determine whether the overall rate of response could be described as the outcome of two independently acting factors, namely, rate of response during active periods (r_{RG}) and rate of silent periods (a silent period being arbitrarily defined as a 2-minute period during which no lever or trough response is recorded); (*b*) more recently the extinction scores of animals trained under continuous and fixed ratio reinforcement (Jenkins & Stanley, 1950) was recorded on SETAR, an electronic serial event timer which issues printed scores (1/100th second). (The machine was loaned to the writer by A. T. Welford and built by Dr. N. Welford, Cambridge.) The results

of these investigations will shortly become available. The present report concerns (a) only.

Rate of Response and Inter-Response Times

Let us assume that the exponential equation, as given by Ellson (1939), gives the best fit to the cumulative response curve for *individual* performers during a period of free-operant extinction. Further, let us assume that $Y = f(X)$ may be transformed $X = f(Y)$ without making any additional assumptions, so that the equation

$$r = R(1 - e^{-bt})$$

may be transformed to

$$t = \frac{1}{b} \log_e \left(\frac{R}{R - r} \right) \qquad \cdot \cdot \qquad \cdot \cdot \qquad \cdot \cdot \qquad (3)$$

i.e. the time interval between successive responses increases with the proportion of the Reserve (R) to the number of responses already made (r). (The curve is positively accelerated.)

This rational derivation from Ellson's exponential equation may then be experimentally tested by plotting successive response intervals as a function of the ordinal number of trials for either individual subjects, or if required for a group of subjects. In the absence of the necessary information for this task, an alternative, indirect approach may be made. An extinction trial may be fractionated into *n*-periods of equal length and a frequency count undertaken for each period. From equation (3) one would predict that the series of scores gradually converge to a set of zero entries (i.e. where $r > R$). On the other hand, on the basis of our previous study (Hurwitz, 1954) we would predict a different outcome, namely (1) that zero entries occur throughout the series though with increasing frequency. This bears on our contention that extinction trials are composed of *active* and *silent* periods; (2) that the rate of response during *active* periods is declining at a considerably slower rate than is predicted from equation (3).

II

THE EXPERIMENT

Subjects: Eleven, three-month-old, experimentally naïve male hooded rats, drawn from the small inbred colony maintained at the Department of Psychology, were used in this study.

Apparatus: The apparatus consisted of a modified Skinner-box housed in a semi-soundproof chamber. As previously reported (Hurwitz, 1955) the lever responded to a pressure of 2–3 gms. It was mounted 3 in. above floor level, 5 in. to the right of the food-trough. The food-trough was shielded by a plastic swinging door which had to be nuzzled open before food could be obtained. The door was fitted with a cam bearing upon a relay contact, so that food-trough responses could be recorded. The circuits of the control equipment were designed to deliver one pellet of food at a time. When the lever was depressed it operated (i) a silent food delivery machine, (ii) a buzzer placed outside the box.

Records: A three-pen paper-tape recorder run at 10 cm./sec. provided us with the required information.

Habituation and feeding schedule: Two weeks before training, the S's were placed into a feeding-fasting schedule apparatus which automatically made food available for two hours, at the time set aside for experimental trials. (Wasservogel & Hurwitz, 1957.) The animals remained in these cages until the conclusion of the experiment.

Training: Training was completed within three trials. On the first trial S was placed into the apparatus and allowed to retrive 10 pellets from the food-trough. A further 20 pellets were delivered one by one by the experimenter, each presentation being preceded by a buzz lasting 0·5 seconds. Thereafter whenever S moved towards the lever, E sounded the buzzer and simultaneously operated the food machine. Ten assisted trials were given and S was retained in the apparatus until it had secured a further 10 pellets without any assistance. On the second and third trial, 30 and 60 pellets had to be obtained. If S took

longer than 40 minutes, it was withdrawn from the experiment. No S failed to satisfy this criterion.

Experimental Extinction: On the fourth day of the experiment the feeding machine was emptied except for three pellets. The trial lasted for 1 hour.

III

RESULTS

The hypotheses underlying the present study were tested by a series of rank-correlation tests on lever scores only. Further, the theoretical variance and obtained variance of scores for each animal were tested by an F-test in order to establish whether the scores conform to the predictions derived from a continuous function or whether they are bimodally distributed.

Partial analysis of trough and lever-scores have also been undertaken. Unfortunately, due to an occasional break-down of the recording pen attached to the trough, these data are incomplete. As a first step, we tested the hypothesis that the *frequency of response decreases throughout the trial.* The frequencies of lever-responses for successive two-minute periods for each subject were ranked and sums of ranks obtained. On the assumption that sums of ranks decrease, a ranking test was undertaken yielding a value of rho (corrected for ties) of 0·63, which is significant beyond the 0·01 level of significance. It therefore seems that our subjects performed in a manner consonant with predictions derived from equation (2) (*see* Introduction).

TABLE I

LEVER PRESSING RESPONSE FREQUENCIES: INTERVALS OF TWO MINUTES

Animal No.	2	4	6	8	10	12	14	16	18	20	22	24	26	28	30	32	34	36	38	40	42	44	46	48	50	52	54	56	58	60
1	7	15	19	13	0	0	32	23	34	12	18	0	1	0	12	7	0	11	12	19	0	10	9	0	5	3	0	0	7	0
2	20	40	7	16	14	18	18	0	9	0	9	11	28	13	0	17	11	0	0	0	0	26	6	0	17	20	0	4	5	0
3	21	21	17	5	16	14	0	17	5	39	54	2	10	0	0	15	0	21	0	0	1	12	13	9	7	0	5	31	17	0
4	16	21	6	0	13	12	0	0	2	12	0	4	0	3	1	15	3	4	0	13	3	0	1	9	7	0	0	11	7	17
5	12	16	15	6	12	27	0	0	0	1	21	7	0	12	0	0	23	51	0	0	7	40	1	18	22	0	0	0	7	0
6	11	19	17	14	0	2	0	7	7	8	1	0	5	5	4	0	9	0	0	4	17	0	0	4	6	0	4	0	0	0
7	13	8	38	22	10	2	16	1	0	13	35	13	6	0	0	1	2	0	1	0	0	7	2	0	0	0	0	4	8	0
8	20	26	11	5	1	15	10	0	10	9	3	11	10	2	0	17	3	0	1	20	27	0	0	0	0	0	0	0	18	12
9	18	0	7	2	5	5	15	3	3	0	0	5	4	0	0	0	9	12	1	0	0	7	0	0	0	0	27	12	30	11
10	14	17	3	0	0	10	17	16	7	0	13	11	13	0	0	11	8	0	0	0	7	1	0	0	0	12	11	3	0	0
11	13	14	6	1	4	17	1	16	7	11	10	22	20	9	3	21	0	1	11	8	1	13	0	0	0	13	8	0	0	4

We next turn to our hypothesis that *extinction trials may be separated into "silent" and "active" periods.* In order to support this hypothesis it must be shown that the trend in scores exhibited in our previous analysis is an *artefact* arising from either of two causes: (*a*) the process of averaging, (*b*) sampling. For purpose of testing the hypothesis all cell entries of "0" and "1" were regarded as indicative of silent-periods and conversely all cell entries of above 2 as active-periods. Our decision to regard "1" entries as silent requires some justification. Inspection of Table I shows 18 such entries, 16 of which lie adjacent to cells classifiable as active-periods. We have

examined the records carefully in order to establish whether the "1" entries may legitimately be regarded as members of the set of responses in the adjacent cell and have found, by applying a criterion of 30 seconds that 15 of the entries conform to this requirement.

The mean response frequency for successive 2-minute periods was calculated by the formula $\sum \dfrac{(C - n)}{A}$, where C refers to the response score, n to the total number of "1" entries and A to the number of active periods for each column in Table I. These means were subsequently ranked and tested against the hypothesis that the response rate was declining throughout the extinction trial. Since rho = 0·19, we are not entitled to reject the null-hypothesis. Thus, our test reveals no significant decline in response-rate, as demanded by, for example, Hullian theory, and a strict interpretation of equation (3). Our final test concerned the silent entries. Silent periods were totalled for each 2-minute period, the totals were ranked and the ranks tested against the *hypothesis that silent periods are increasing.* Since rho = 0·73, the hypothesis is confirmed ($p = 0·01$).

FIGURE 1
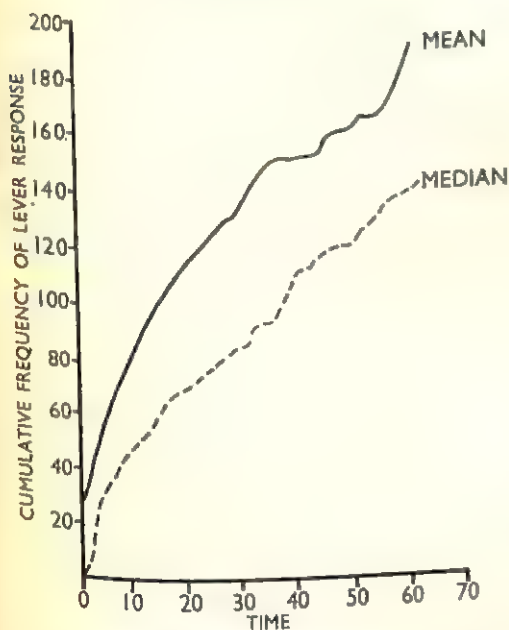
FIGURE 2
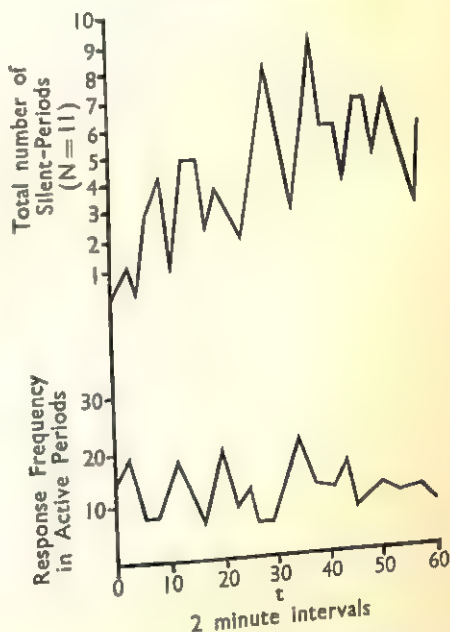
In Figure 1 the mean and median cumulative response scores for the whole group is shown: Figure 2 shows the non-cumulative response frequencies for active periods only, and thus demonstrates graphically our main contention that the exponential character of mean and median curves is largely determined by the interaction of two functions, relating to active and silent periods.

IV

DISCUSSION

Thus far, the statistical analysis has given indirect support to our criticism of the use of exponential monotonic functions for describing the distribution of response intervals, i.e. rate of response, during operant extinction trials. Rather, it is characteristic of the animal to respond sporadically during such trials.

The sporadic character of the behaviour may be brought to law. We have already indicated the traditional manner of doing this: means computed for successive corresponding points on the time-scale are plotted cumulatively (see Figure 1). This results in a smooth function which seems to approximate rather closely to the curve idealized in Hull's (1943, 1952) theory and given explicit expression in Ellson's paper (1939).

In contrast to the traditional approach, the behaviour may be visualized as a product of a set of interacting variables whose values are being in part affected by the behaviour, as also by the outcome of behaviour. The model is stochastic in nature, conceived in terms of a probability calculus, and requires specialized techniques for handling data. Unfortunately, the data provided by the present experiment does not permit us to make full use of the inherent possibilities in such a model, but Audley 1956) has recently shown how data derived from a similar experimental set-up could be handled by stochastic methods.

It seems to us, that short of a searching and expansive theoretical discussion of the analysis of operant behaviour, particularly regarding the relation of acquisition to extinction, we should confine ourselves to a number of issues directly related to our present results. These issues are (a) the effect of processing raw scores in a Skinnerian situation, (b) Active and Silent periods, and (c) the response rate.

(a) Processing Raw Scores:

Sidman (1952) recently raised the question ". . . whether, given a mean curve, the *form* of the individual curve is uniquely specified." As Estes (1956) has pointed out, this question hides three separate issues: according to Estes, some functions are unmodified by averaging, in other cases averaging complicates the interpretation of parameters, but leaves the form unchanged, whilst a third class of functions, of which the exponential growth curve is an example, an averaging procedure modifies the *form* of the function. From our results we see that averaging extinction scores in an operant situation like the Skinner box, transforms the functions; more specifically it has led to the supposition that the best fitting function of the averaged scores are psychologically meaningful and define the properties of indepedent variables like "drive," "habit strength," etc. One may speculate as to which came first: the functional form or the parameters determining the curve. Skinner (1938), for instance, deliberately by-passes the obvious irregularities inherent in the individual (cumulative) response curves and recommends drawing an envelope over the curves so as to smooth them. This recommendation was based on the assumption that rate of response is the critical dependent variable, so that specifying the slope of the curve was of primary importance. Experimenters concerned with the validation of Hullian principles have probably taken heart from Skinner's recommendation and have used averaged data only. It is useful to reopen this issue and inquire once again to what extent one's theoretical preconceptions determine the choice of averaging technique (and processing of data in general).

(b) Active and Silent Periods:

Strictly speaking, *active* and *silent* periods are distinguished on the basis of a criterion *inter-response interval*. In the present instance, this method was waived in favour of the cruder procedure of dividing the total trial period into 2-minute periods and making response-frequency counts. Thus, a zero score for a period was taken as suitably dividing a set of responses into two active periods interspersed by one silent period. There is a serious limitation to this method. A zero entry may in fact indicate a silent period of up to 6 minutes, since the two active periods (minimum

scores of 2 each) may have occurred at the tail-ends of the "active" 2-minute periods. This sets a distinct limit to the interpretation of our results. However, our selection of a 2-minute interval was not arbitrarily determined. A pilot study on a small group of animals in which the actual inter-response intervals were obtained yielded the following result: the probability of two consecutive 2-minute inter-response intervals was compared to the probability of securing a single 2-minute silent period. On the basis of this computation it appeared that there is a high probability of obtaining one 2-minute inter-response interval. In other words, there was a very small chance of getting a set of *consecutive* responses spaced at 2 minutes or more, as against a relatively high chance of getting a 2-minute inter-response interval between two successive responses. This is clearly an argument for grouping, provided the demarcation is kept at below 2 minutes. The results allow for the following interpretation: Given the occurrence of a response, there is a high probability that at least one further response would occur within the next 2 minutes; however, given n responses, there is a high probability that the $n + 1$ response would not occur within the next 2 minutes. This latter probability increases as a function of $R_g r$ response-groups (Hurwitz, 1954).

(c) *The Response Rate:*

A previous study (Hurwitz, 1954) described the progressive change in response durations during experimental extinction trials. It was found that the average duration per response increases throughout a trial. Furthermore, the nature of the increase could be related to the secondary stimulus conditions which had become associated with the performance of the response during training trials.

Now from this finding two divergent predictions may be made: (a) if rate of response is based on the frequency of response per unit time, the rate of response per respondent period should decline. (b) If, however, rate of response is computed on the basis of inter-response intervals, the rate should be almost linear since individual response durations are increasing. The curvilinear relation following from procedure (a) is explicable in terms of the rate of response being a joint function of inter-response intervals and increasing response durations. In actual fact, the situation is more complicated.

Although Figure 2 suggests that the rate of response per active period is relatively steady throughout the time-range sampled, Table I tells a different story. When we consider the scores of individual subjects during active periods, the function expressing rate of change is *not* linear. Rather, the following description of successive cell entries applies: the probability of a high or a low score is relatively equal throughout the time-range sampled. Thus, an animal may obtain a high or low score at any point of the range. It is, however, highly improbable that it will respond with equal frequency throughout all active periods.

We have undertaken the following test for our suggestion that the response scores of individual animals does not conform to the requirements of the exponential growth function model, which assumes a continuous distribution of scores, but that, on the contrary, the data is bimodally distributed:

The data in Table I was regrouped so that response frequencies were obtained for successive 6 minute periods. By employing the maximum likelihood procedure outlined in Bush and Mosteller's monograph, *Stochastic Models for Learning* (1955), we were able to test the variance of the resultant array of scores against a theoretical variance. This procedure required use of the function gr (d)—section 9·10 and Table C. Given the initial response rate (for the first 6 minutes) and the assumption that the rate of response declines in a monotonic manner, the two variances should not yield a significant difference on the F-test. Yet our computations revealed that

F exceeded a value of 5 for all animals and attained values as high as 20, i.e. the differences were highly significant.

Consequently our main contention, that the mathematical characteristics of operant behaviour during experimental extinction trials are insufficiently described by the quantitative techniques recommended by Hull and others, finds support in the analysis of the present set of data.

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ONE TRIAL LEARNING AND THE SUPPRESSION OF INTERFERENCE

BY

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An experiment was made to investigate the conditions under which a subject who has learned a series of items in one order can relearn the same items in a different order in one trial. The hypothesis tested was that one-trial learning is ensured in this situation if interference between the two tasks is reduced to zero. It is shown that complete suppression of such interference, obtained by overtraining on the task which is learned first, is not sufficient to ensure the learning of the second in one trial. Comparison of the present experiment with a similar one reported by Mandler and Heinemann (1956) suggests the hypothesis that the discovery of order in the stimulus situation is necessary, in addition to the suppression of interference, if one trial learning is to occur. The relation of the results to the problem of negative transfer is also discussed.

I

INTRODUCTION

While it has been generally assumed that previous experience with a *variety* of problems of the same class is necessary for the development of the capacity to solve new problems in that class in one trial, a recent experiment by Mandler and Heinemann (1956) has indicated that overlearning a *single* problem may, in certain circumstances, produce the same effect. One characteristic effect which the developing learning set and increasing amounts of overlearning have in common is the progressive suppression of interference by previously learned associations with the learning of the new task (Underwood, 1951; Riopelle, 1953). In the absence of any explanation of the learning set phenomenon on the one hand, and of any information concerning the order in which different types of error are eliminated during overlearning on the other, it was decided to explore further the relation between one-trial learning and the suppression of interference. The main purpose of the experiment described below was to find out whether maximum positive transfer (i.e., one-trial learning) from one task to another can be produced by overlearning one to the point where overt interference between the tasks is eliminated.

A secondary purpose of the experiment was to check on a discrepancy between Gagné, Baker and Foster's (1950) theory of transfer and the results of experiments relevant to that theory. Gagné *et al.* have argued that maximum amounts of negative transfer should be obtained when the second task involves a recombination of the stimuli and responses used in the training task (the A–B, A–B_R paradigm). While this prediction has been confirmed by Porter and Duncan (1953) in an experiment using paired adjectives, those who have used other materials have failed to obtain significant amounts of negative transfer with this paradigm (Siipola and Israel, 1933; McClelland, 1943; Mandler, 1954; Mandler and Heinemann, 1956).

II

METHOD

Task and apparatus

Nine stimuli and nine responses were chosen from the selection of disyllabic adjectives provided by Hilgard (Stevens, 1951, pp. 548–552). Two lists of paired associates were made up from these adjectives, the second list being a re-pairing of the stimuli and

responses of the first list. These lists, together with the instructions, were then recorded on a tape-recorder, which was used as an auditory memory drum. The stimulus adjective was given, then four seconds later this adjective was repeated together with the response adjective with which it was paired. Four seconds later the second stimulus was given, and so on to the end of the list. The order of the paired associates was randomized in such a way that every pair followed every other pair an equal number of times (Williams, 1949). One full replication of the list required 18 trials. Eight seconds elapsed between the end of one trial and the beginning of the next, and two minutes between the first and second list. The anticipation method was used.

Subjects

The subjects were students and members of staff of the Department of Psychology. All had had previous experience of rote learning experiments. None knew the purpose of the experiment, or that after learning the first list they would have to learn a re-arrangement of that list. They were divided into three groups, 10 in each group.

Instructions

All the subjects were given standard paired-associate instructions before learning the first list. Before learning the second list, they were told that this list would be composed of the same stimuli and the same responses as were used in the first list, but that they would be paired differently.

Training conditions

Group C learned the first list to a criterion of one errorless trial. Group OL practised the list for 18 trials and Group OOL for 36 trials. Learning of the second list was carried to a criterion of one errorless trial. Half the subjects in each group learned one list first and the other half learned the other list first.

III

RESULTS

Training results

Analysis of variance of the training data, using trials to criterion and errors to criterion, indicated that there were no significant differences between the three groups in learning the first list, or in the difficulty of the two lists. An average of 21 errors was made in learning the training list, and an average of 6·6 trials taken to reach a criterion of one errorless trial.

TABLE I

MEAN ERRORS AND TRIALS TO CRITERION (ONE ERRORLESS TRIAL) ON TRAINING AND TRANSFER LISTS, TOGETHER WITH PERCENTAGE SAVING, AS A FUNCTION OF AMOUNT OF PRACTICE ON TRAINING LIST

Amount of training on first list	List	N	Errors		% Saving	Trials		% Saving
			Training	Transfer		Training	Transfer	
To one errorless trial	1	5	14·8	10·4	24·0	5·8	4·8	7·0
	2	5	32·4	25·6		8·4	8·4	
18 trials	1	5	20·0	13·2	17·0	6·6	6·0	−2·0
	2	5	19·2	19·2		6·2	7·0	
36 trials	1	5	19·8	8·6	38·0	6·4	4·2	24·0
	2	5	20·0	16·0		6·2	5·4	

Transfer results

Analysis of variance of the transfer scores (raw training score minus raw transfer score), using trials to criterion and errors to criterion, indicated that there were no significant differences between the groups in learning the transfer list, or in the difficulty of the two lists.

The trends for total errors and total trials to reach criterion on the transfer list (Table I) parallel those described by Mandler (1954). Moderate overlearning (Group OL) produces less positive transfer than learning to the criterion of one errorless trial (Group C); in neither case does the amount of transfer differ significantly from zero. Extreme overlearning (Group OOL) gives rise to a significant amount of positive transfer (two-tailed test), as measured both by the saving in total trials ($t = 2.28$; $p = 0.05$) and in total errors ($t = 2.47$; $0.05 > p > 0.02$).

TABLE II

MEDIAN FREQUENCY OF DIFFERENT TYPES OF ERROR MADE IN LEARNING TRAINING AND TRANSFER LISTS AS A FUNCTION OF AMOUNT OF PRACTICE ON TRAINING LIST

(a) Training Lists.

Amount of practice on training list	Type of error*			
	Omissions	Intralist Intrusions	S-R Reversals	Others
To criterion of one errorless trial	10.5	2.5	0.0	0.0
18 trials	13.0	2.5	0.0	0.0
36 trials	11.0	3.0	0.0	0.0

(b) Transfer Lists.

Amount of practice on training list	Type of error*				
	Omissions	Intralist Intrusions	S-R Reversals	Interlist Intrusions	Others
To criterion of one errorless trial	10.0	2.5	0.5	2.0	0.0
18 trials	9.0	2.0	0.0	0.5	0.0
36 trials	5.0	1.5	0.0	0.0	0.0

* Omissions are defined as a failure to respond; intralist intrusions as giving the response which would be correct for a stimulus in the list other than that actually presented; S-R reversals as giving another stimulus as a response; interlist intrusions as giving in the transfer list the response learned in the training list.

In Table II, the errors made in learning the training and transfer lists are broken down into five categories: intrusions from training to transfer list, omissions, stimulus-response reversals (i.e., giving another stimulus adjective as a response), intralist errors (i.e., giving a response which would be correct for some other stimulus, but which was incorrect for the presented stimulus in both the training and the transfer list) and "other" errors (i.e., giving a response not used in the experiment). It will be seen from this table that 36 trials of practice on the training list were sufficient to reduce the median number of interlist intrusions to zero (only one such intrusion was observed), but not sufficient to eliminate other sources of error.

IV

DISCUSSION

(a) Overlearning and the suppression of intrusions

Mandler and Heinemann (1956) have succeeded, by overtraining on one paired-associate list, in reducing the phenomenal complexity of a subsequently presented list of paired associates to the point where the latter was learned in one trial. The results of the present experiment indicate that their success was not due solely to

the fact that overtraining on the first task prevents that task from interfering with the second. When first task learning was sufficient to eliminate intrusions into the second task, omissions were still observed with a median frequency of 5 and intra-list errors with a median frequency of 1·5.

Since, so far as we are aware, the only known concomitant of overlearning is a reduction in intrusions from the overlearned task, it is necessary to look elsewhere for an explanation of Mandler and Heinemann's results. Comparison of their experiment with our own suggests that at least two conditions may be necessary for one-trial learning. Firstly, the material to be learned must be free from inter-ference by other, previously learned associations. One way in which this may be achieved is by overlearning these associations, a condition provided both by our own experiment and that of Mandler and Heinemann. Secondly, the material must evoke in the subject some previously learned order or syntax, so that the ordering imposed by the experimenter upon the material to be learned becomes, so far as the subject is concerned, irrelevant. This condition is immediately provided for in Mandler and Heinemann's experiment, but less obviously so in our own. Their stimuli were integer numbers, which had already been learned in a unique order before the experiment began. Thus it is possible that in addition to learning, during practice on the first task, that LFH is the response to 7, KGR the response to 9, BQX the response to 3 and MJS the response to 4, Mandler and Heinemann's subjects also learned that the "first" response (i.e. the response to the lowest stimulus number) is BQX, the "second" response is MJS, etc. Each response is now uniquely defined by reference to its position among all the other responses, and its recall no longer depends upon the order in which the experimenter chooses to present the stimuli (which will, of course, differ from trial to trial). When the subject is presented with the re-paired list (or with any other list to which the same rule applies), he simply has to learn his own ordering of the responses, rather than the stimuli and the responses and the order in which they are paired by the experimenter. If the new ordering, for example, is 3-LFH, 5-MJS, 4-BQX, etc., he has merely to remember that the "first" response is LFH, the "second" BQX, the "third" MJS, etc. When the economies which ensue from the elimination of interference between the responses and from the imposition of a syntax upon the stimuli are transferred to the learning of the second list, one-trial learning of the latter may be accomplished on the basis of immediate memory alone.

In our own data, there is evidence that all except one subject imposed their own organization upon the material to be learned. This took the form of inventing stories to connect stimuli with responses. Thus one subject said that he found it easy to link *Bearded* with *Single*, "because I didn't think a wife would let a man grow a beard," *Formal* with *Modern* by saying "in modern times men wear formal clothes," *Absent* with *Modern* by "the absence of manners in modern times," and *Formal* with *Utmost* by "if there is a formal occasion, one must be on one's utmost best behaviour." While these "well organized, though quite unusual wholes" as Köhler (1947, pp. 265-266) has called them, have properties which may facilitate the connecting of stimuli with responses (their fantastic nature presumably renders them immune from retroactive inhibition), they lack an essential characteristic of the number series employed by Mandler and Heinemann. As they stand, they do not permit the subject to integrate the stimuli into a unique order.

(b) *The A-B, A-B_R paradigm and negative transfer*

A secondary purpose of the experiment was to test Gagné, Baker and Foster's (1950) analysis of the conditions which maximize negative transfer. As we have

already noted, only one (Porter and Duncan, 1953) of the five relevant experiments so far reported in the literature has produced results consistent with this analysis. Porter and Duncan differed from the other experimenters in that they used paired-associate adjectives, but our failure to find any evidence of significant amounts of negative transfer indicates that this difference is not responsible for the fact that their results are exceptional. In general, we may conclude that negative transfer, in the sense of an increase in the number of trials required (or the total number of errors made) to learn a second task to an errorless criterion as a result of having learned a previous task, is a rare event. A satisfactory account of transfer will have to give reasons for the fact that, under a wide variety of conditions, interference is demonstrable only on the first few trials of the transfer task.

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SOME EFFECTS OF THIAMINE DEFICIENCY AND REDUCED CALORIC INTAKE ON AVOIDANCE TRAINING AND ON REACTIONS TO CONFLICT

BY

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The present experiment was designed to test four predictions based upon results of our earlier study (Knöpfelmacher *et al.*, 1956) in which we observed some effects of thiamine deficiency and reduced caloric intake on "behaviour under stress" and on learning. The first two predictions stated that thiamine deficiency of a degree not severe enough to produce polyneuritis (*a*) would not affect the efficiency of conditioning and (*b*) would not be associated with exaggerated reactions to conflict. The third prediction arose from our previous observations of the effects of "irrelevant drives" on learning and stated that animals on reduced caloric intakes would condition more rapidly as a group than would animals fed *ad libitum*. The fourth prediction was that reduced caloric intake would not be associated with increased susceptibility to stress.

The experiment was identical in its design to our previous study. It required one experimental and two control groups, all matched by a littermate control technique. The experimental group was maintained on a thiamine-deficient diet throughout the entire experimental period. A caloric control group received adequate thiamine, but a food intake reduced to that of the vitamin-deficient animals. A general control group received adequate thiamine and an unrestricted food intake. Because of the effects of thiamine deficiency on caloric intake, food-hunger was never used as a form of motivation. Behaviour in three different situations was studied: in an instrumental conditioning situation involving jumping to a platform to avoid shock; in an avoidance-avoidance conflict situation; and in a second instrumental conditioning situation requiring lever-pressing to avoid shock.

The results confirmed some of our predictions, but not others. In general, they indicated that thiamine deficiency of the level we induced did not affect the efficiency of conditioning. This conclusion conforms with the findings of our earlier experiment that the deficiency did not affect maze or discrimination learning, but it is contrary to the observations of certain other investigators. The evidence did suggest that thiamine deficiency was associated with exaggerated reactions during exposure of the animals to conflict, deficient animals tending to show greater response rigidity and "displacement activity." The divergences in the behaviour of these animals and their paired caloric controls became more noticeable as the number of conflict trials increased. Our prediction that reduced caloric intake would be associated with more rapid conditioning was confirmed. We believe this result suggests that "irrelevant drives" were operating in a manner similar to that reported in our previous study. The overall results support our fourth prediction that reduced caloric intake would not affect reactions to conflict. However, an analysis of behaviour trends during the conflict trials suggests that reduced caloric intake may have affected reactions early during exposure to conflict and only became differentiated from the effects of thiamine deficiency as the duration of exposure increased.

I

INTRODUCTION

Developments in our knowledge of the pharmacology and physiology of the nervous and endocrine systems are continually providing psychologists with new techniques in their search for bodily structures and functions underlying behaviour. Among these are techniques for interfering with the intermediary metabolism of

tissues, techniques which involve altering specific steps in important biochemical events and thereby creating "biochemical lesions" (Peters, 1948). These biochemical events depend upon the actions of certain essential enzymes and co-enzymes (Baldwin, 1953). The present study is the second in which we have interfered with the action of thiamine, a constituent of a co-enzyme important to the metabolism of carbohydrates in the nervous system, and have observed effects on certain aspects of behaviour.

In 1937 Stevens indicated clearly how thiamine deficiency might have effects on behaviour by altering brain chemistry, by creating structural lesions in the nervous system, or by non-specific bodily changes resulting in decreased amounts of available energy subsequent to inanition, lowered metabolic rate, decreased body temperature, loss of appetite, etc. His and several other studies have been concerned with the effects of such deficiency on the adaptive behaviour of infrahuman animals, including maze learning and conditioning. The results of these studies have, in some instances, shown decreases in the efficiency of performance and, in other others, no significant effects. We have suggested (Knöpfelmacher *et al.*, 1956) several possible reasons for these equivocal results and would like to call attention briefly to three which are particularly relevant to the experimental design in our present study. First, since thiamine deficiency is associated with decreases in food intake, it is important to compare the behaviour of each deficient animal with the behaviour of a paired animal maintained on the same caloric intake but with adequate thiamine supplements; such a control is necessary in order to differentiate effects of thiamine deficiency from effects of reduced caloric intake. Second, the known effects of thiamine deficiency on caloric intake, with anorexia as one of the symptoms, make it undesirable to use food deprivation as a form of motivation. Third, extreme and prolonged thiamine deficiency produces peripheral degeneration in both sensory and motor nerve fibres. These changes occur much later than changes in the metabolism of the central nervous system. Therefore, if central nervous system metabolism is to be altered without producing peripheral symptoms, the degree of thiamine deficiency must be controlled. In the present experiment we used the same procedures in regard to these three critical conditions as those employed in our first thiamine study.

The first study was designed to determine the effects of thiamine deficiency in rats on behaviour under the stress of exposure to an insoluble problem situation and on maze learning. Contrary to the results of several previous studies, we found no significant effects of the deficiency prior to the onset of polyneuritis with its symptom of impaired motor coordination. The results suggested that alterations in the normal carbohydrate metabolism of the central nervous system resulting from thiamine deficiency may not affect behaviour until they are sufficiently severe to produce the symptoms of polyneuritis.

In the present experiment we have studied the effects of thiamine deficiency and reduced caloric intake on two other forms of behaviour. The first involved the establishment of instrumental conditioned responses of the avoidance training variety (Hilgard and Marquis, 1940). The second provided measures of reactions during and after exposure to stress involving a conflict between two avoidance responses (Russell, 1953). The experiment was designed to test four main predictions. It was predicted that decreased carbohydrate metabolism in the central nervous system resulting from thiamine deficiency of a degree not severe enough to produce polyneuritis (a) would not affect the efficiency of conditioning and (b) would not be associated with increased signs of susceptibility to stress. The third main prediction was based upon observations in our previous thiamine experiment that the performance of the caloric control group appeared to be influenced by an interaction

between the two drives, escape from water and food-hunger, even though the latter was never reinforced at the completion of any trial. This prediction stated that the caloric control animals would condition more rapidly as a group than would the general control animals. The fourth main prediction was that the caloric controls would show no increased susceptibility to stress when compared with the general control animals.

II

METHOD

The experimental design is shown in Table I and is in principle identical with the design used in our previous experiment (Knöpfelmacher *et al.*, 1956). It required three groups of animals: a general control group, a thiamine-deficient group, and a caloric control group. The latter received the same amount of thiamine as the general control group, and served as a control for the effects of reduced caloric intake in the thiamine-deficient group. Only the general control group received an *ad libitum* supply of all the necessary dietary components.

TABLE I
EXPERIMENTAL DESIGN

Group	Diet	Phases		
		1	2	3
I	Thiamine - supplemented *ad libitum*	Conditioning: avoidance training 1	Conflict	Conditioning: avoidance training 2
II	Thiamine-deficient *ad libitum*	Conditioning: avoidance training 1	Conflict	Conditioning: avoidance training 2
III	Thiamine - supplemented caloric control	Conditioning: avoidance training 1	Conflict	Conditioning: avoidance training 2

Subjects

Twenty-four male hooded rats of highly inbred Lister stock from the animal colony of the Department of Nutrition, Queen Elizabeth College, University of London, served as subjects. Males from litters weaned at 23 days of age were selected and were assigned to the three groups in accordance with customary litter-mate control techniques. They were started on the feeding schedules and diets appropriate for the groups to which they belonged when their median age was 51 days and they continued on these diets until the experiment was completed.

As will be apparent as soon as the results of the study are discussed, half of the general control animals failed to reach the criterion for conditioning in the 100 trials given during the first phase of the research. These four had to be discarded since their participation in subsequent phases depended upon the attainment of this criterion. Therefore comparisons of behaviour during phases 2 and 3 involved four animals in the general control group and eight in each of the other two groups. The fact that these animals feeding *ad libitum* on a fully adequate diet were slower to condition than the other animals with caloric intakes reduced experimentally or as a result of thiamine deficiency is in accord with our third main hypothesis regarding the interaction of drives. However, as we will point out in discussing our results, there are other possible hypotheses which can only be tested by further research.

A general routine for handling the animals was followed daily throughout the experiment. In the morning they were weighed and fed thiamine supplements where required, and the food left in their feeding jars was also weighed. In the afternoon the animals

were taken individually from their cages, given the appropriate trials for the day, and returned to their cages.

Diet and growth

The compositions of the diet and of the various supplements were identical with those used in our previous experiment. Group I, the general control group, was allowed an *ad libitum* food intake, supplemented with adequate doses of thiamine (20 μg. daily). Group II was maintained on an *ad libitum*, thiamine-deficient diet. Their daily intake of thiamine was 5 μg., an amount which previous experience suggested would give sub-optimal growth but would not lead to polyneuritis or death for at least several months (Yudkin, 1949). Animals in Group III received daily the amount of food consumed on the preceding day by their respective litter-mates in Group II and their diet was also supplemented with 20 μg. thiamine daily.

The animals were weighed twice a week and their food intake was checked daily to the nearest gramme. Growth rates of animals in Groups II and III, as measured by body weight, became significantly less ($p = 0.02$) than those of Group I when the animals' median age was 85 days. The food intake of Group II and, by experimental artifact, also of Group III dropped considerably below the corresponding intake of Group I when the growth rates became differentiated. Curves for growth rates and for food intake were so similar to those of our previous thiamine study (1956) that we are not reproducing them here. Phase I of the experimental design was begun after growth rates had continued to be significantly different for 12 consecutive days.

It is important to emphasize that food-hunger was never used as a form of motivation in the present research. The reasons for this are apparent and arise primarily from, first, the rigorous dietary controls required by the experimental design and, second, the effects of thiamine deficiency, e.g. anorexia, on caloric intake.

Apparatus

The apparatus used during all phases of the research was designed by Dr. F. Knöpfel-macher (now of the Department of Psychology, University of Melbourne). It consisted essentially of four different, but connected, units: a conditioning box, a shock control unit, an electrical timer and an ink-writing polygraph. The main features of the condition-ing box, from the point of view of the present experiment, were the two grids serving as floors for the main unit and for the retractable platform, and the retractable lever. The
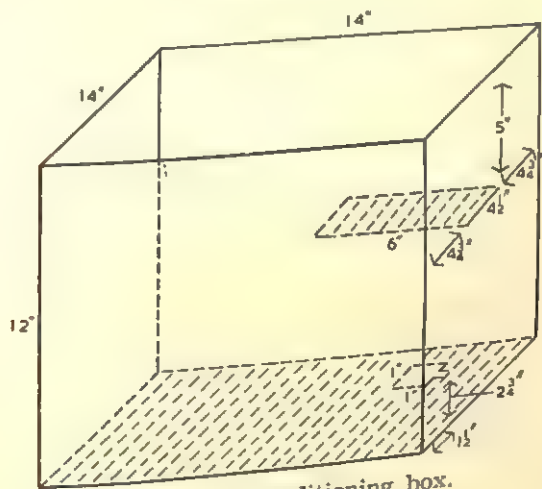
FIGURE 1

Diagram of conditioning box.

dimensions of these are shown in Figure 1. The distance from the main floor grid to the platform was 7 inches. The onset and duration of the shock through the two grids were controlled manually by the experimenter. The intensity of the shock was regulated electronically at 1·0 to 1·5 milliamperes by a constant current control unit. The ways in

which these various features of the apparatus were used during the different phases of the experiment are described in the following paragraphs.

Avoidance training 1

As indicated in Table I, during the first phase of the experiment all animals underwent conditioning of the avoidance-training variety. Their median age at the beginning of this phase was 121 days. Twenty seconds after the animal was put into the box the platform was inserted, the insertion serving as the conditioned stimulus. This was followed 10 seconds later by the onset of the shock delivered through the floor of the main unit, which continued for 60 seconds. An animal could avoid the shock by jumping on to the platform within the 10-second pre-shock interval or could escape by jumping after the onset of the shock. One trial daily was given and the criterion for conditioning was three consecutive trials in which jumping occurred within the 10 seconds. Four animals in the normal control group failed to reach this criterion in 100 trials, when this phase of the experiment was terminated. Records were kept of each animal's response to the conditioned stimulus on each trial and of the latency of this response.

Conflict

In the second phase of the experiment the animals were exposed to conflict of the avoidance-avoidance variety (Miller, 1944). Having learned to jump to the platform in order to avoid shock, they were now placed in a situation in which a jump also led to shock. Thus both of the responses available to the animal led to punishment and avoidance. As in phase 1, 20 seconds after the animal entered the box the platform was inserted and 10 seconds later shock was delivered to both the floor and the platform for a period of 60 seconds. Each animal had forty such conflict trials.

Our records of behaviour during each trial included a measure of the latency of responding and the nature of the response, e.g. jumping or not jumping to the platform, and jumping on and off the platform (oscillation). We also recorded any unusual form of behaviour, behaviour which did not occur during conditioning trials in which conflict was not involved. One such form of behaviour, biting the apparatus, was evidenced with striking frequency in one group and will be reported in discussing the results.

Avoidance training 2

In phase 3 a new avoidance training problem was presented. The insertion of the lever, 20 seconds after the animal had entered the box, served as the conditioned stimulus. Ten seconds later shock was delivered through the floor for a period of 60 seconds. An animal could avoid the shock by pressing the lever within the 10-second interval or could escape by pressing after the onset of the shock. One trial per day was given and training was terminated if an animal had not been conditioned in 50 trials. The criterion for conditioning was the same as that in phase 1, e.g. three consecutive trials in which the lever was pressed within the 10-second pre-shock interval. Records were kept of lever-pressings and their latencies, and of the trials during which biting of the apparatus occurred.

III

RESULTS

In analysing the results for significance of differences among the various groups, two distribution-free statistics were used: the Mann-Whitney test (1947) and the Fisher-Yates test (Finney, 1948). "Significance" is defined in terms of the 5 per cent. level of confidence, although p-values are given in the text and tables.

Avoidance training 1

Table II presents the median numbers of trials taken by the various groups in reaching the criterion of conditioning. Group I, the general control group, has been divided into those animals reaching the criterion (Sub-Group IA) and those failing to reach it within the arbitrary limit of 100 trials (Sub-Group IB). Neither of the differences between the thiamine-deficient group, Group II, and the two control Groups IA and III even approached statistical significance ($p = 0.47$ and 0.25, respectively). Although the caloric-control animals tended toward fewer

trials to condition than the normal control Group IA, again the difference was not significant ($p = 0.25$).

TABLE II

AVOIDANCE TRAINING I: TRIALS TO CONDITION

Group	n	Median[3]	Range
IA[1]	4	19.0	1.0–45.0
IB[2]	4	100.0	—
II	8	8.0	1.0–60.0
III	8	5.5	1.0–30.0

[1] The four animals which conditioned within 100 trials.
[2] The four animals which did not condition within 100 trials.
[3] None of the differences among groups IA, II, and III is significant.

Table III shows the median response latencies for the various groups. These results support those reported above for trials to condition, neither of the differences in latencies between the thiamine-deficient animals and the control animals being significant ($p = 0.47$ and 0.36).

TABLE III

AVOIDANCE TRAINING I: LATENCIES OF RESPONSES (SECONDS)

Group	n	Median[3]	Range
IA[1]	4	15.00	4.50–70.00[4]
IB[2]	4	52.50	33.00–70.00
II	8	16.25	4.00–70.00[4]
III	8	11.00	6.50–44.00

[1] The four animals which conditioned within 100 trials.
[2] The four animals which did not condition within 100 trials.
[3] None of the differences among groups IA, II, and III is significant.
[4] This high end of the range in Groups IA and II depends upon one animal in each group which showed long latencies until the last few trials.

TABLE IV

PHASE 2: PERSEVERATION OF THE RESPONSE CONDITIONED IN PHASE I

Group	N	Median[1]	Range
IA	4	33.5	4.0–39.0
II	8	38.5	29.0–40.0
III	8	33.5	28.0–39.0

[1] The difference between Groups II and III is significant ($p = 0.05$, one-tail test) but the other differences are not.
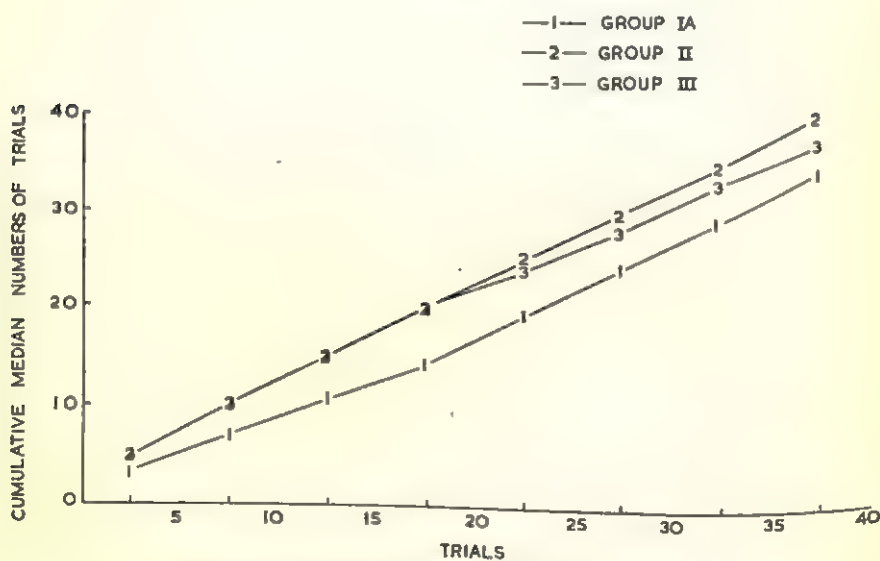
Conflict

Past research has suggested that one characteristic of the behaviour of man and infra-human animals when exposed to a wide variety of stresses is an increased rigidity or perseveration of previously learned responses. In the present experiment, the most recently learned response was to jump to the platform in order to avoid shock. Table IV shows the median numbers of trials during which each of the

groups persisted in making this response while exposed to the avoidance–avoidance conflict. The differences between the caloric control group, Group III, and the thiamine-deficient group, Group II, was small, but statistically significant in the greater perseveration of the thiamine-deficient animals. The performance of the general control group was not significantly different from that of either Group II or Group III ($p = 0.14$ and 0.53, respectively).

TABLE V

PHASE 2: CUMULATIVE MEDIAN NUMBERS OF TRIALS ON WHICH
PHASE I RESPONSE PERSEVERATED

Trials	Group		
	IA	II	III
1–5	3.5	5.0	5.0
6–10	7.0	10.0	10.0
11–15	10.5	15.0	15.0
16–20	14.0	20.0	20.0
21–25	19.0	25.0	24.0
26–30	24.0	30.0	28.0
31–35	29.0	35.0	33.0
36–40	34.0	40.0	37.0

FIGURE 2

Phase 2: Perseveration of Phase I response.

In order to discover any trends in response perseveration which might have occurred during the 40 conflict trials, median numbers of trials on which the phase I conditioned response perseverated were calculated for each group in sets of five trials. These data are given in Table V and Figure 2, cumulative medians being used. The thiamine-deficient animals, Group II, showed great rigidity in responding, persisting in jumping during all sets of trials despite the punishment which inevitably followed. Group III, the caloric control animals, were equally persistent until about midway

through the 40 conflict trials, when they began to show some signs of deviating from their phase 1 response. The general control animals, Group IA, evidenced such deviation from the very first set of conflict trials and showed less behaviour rigidity throughout phase 2. It is interesting to note that the two groups, Groups II and III, whose food intakes and, hence, body weights were significantly lower than the general control group as a result of the experimental treatments, tended to be more persistent in maintaining the phase 1 response.

These differences between the various control and experimental groups are also apparent in terms of the median latencies in making the platform-jumping response. As shown in Table VI, the median latencies for the two control groups were very similar and both were significantly longer than the median latency for the thiamine-deficient animals. Again it appears that the deficient animals tend to be more rigid in their performance of the response conditioned just prior to the introduction of the conflict situation.

TABLE VI
PHASE 2: LATENCIES IN PLATFORM-JUMPING RESPONSE

Group	N	Median[1]	Range
IA	4	26·25	12·0–70·0
II	8	8·25	2·0–29·5
III	8	23·25	11·5–31·5

[1] The differences between Groups IA and II ($p = 0.04$, one-tail test) and between Groups II and III ($p = 0.03$, one-tail test) are significant, but the difference between Groups IA and III is not ($p = 0.29$, one-tail test).

TABLE VII
PHASE 2: CUMULATIVE MEDIAN LATENCIES IN PLATFORM-JUMPING RESPONSE PER FIVE TRIALS

Trials	Group		
	IA	II	III
1–5	49·0	5·0	4·0
6–10	96·0	14·0	15·0
11–15	148·0	27·0	33·0
16–20	204·0	43·5	58·5
21–25	227·0	55·5	109·0
26–30	240·5	64·0	155·5
31–35	250·0	69·5	180·0
36–40	257·0	79·5	203·5

In order to discover any trends in response latencies which might have occurred during the 40 conflict trials, median latencies were calculated for each set of five trials. This breakdown is shown in Table VII and Figure 3, cumulative medians being used. It is immediately apparent that the overall median latencies do indeed cover up differences among the various control and experimental groups. The thiamine-deficient group, Group II, showed relatively little change in response latencies throughout the conflict trials, responding rapidly and consistently to the conditioned stimulus of phase 1 regardless of the punishment following the response. In striking contrast is the general control group, Group IA, in which the conditioned

response of phase 1 was affected within the first five conflict trials as evidenced by a marked increase in response latency. This increase in latency persisted during the first half of the conflict trials and then dropped until, at the end of phase 2, the latency was essentially the same as that for the deficient animals. The caloric control group, Group III, behaved in a still different manner. Its median latencies began at the same level as the deficient group; increased until, after the midpoint of the conflict trials, the latency had reached the long duration of the general control group's initial performance; and dropped again, although not reaching the final level of the other two groups.

FIGURE 3
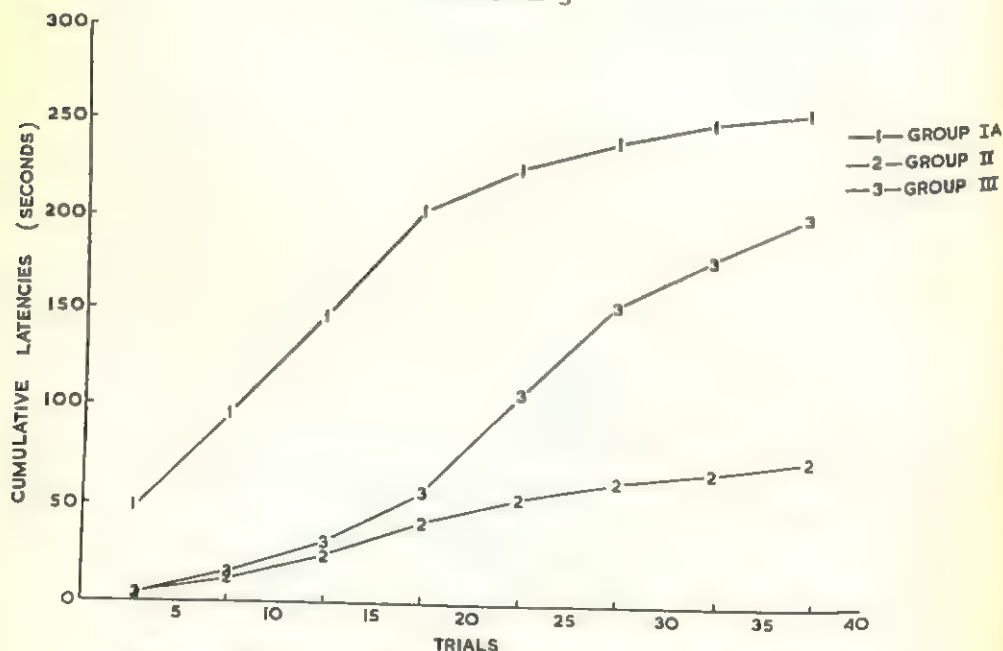
Phase 2: Cumulative latencies in platform-jumping response.

TABLE VIII

PHASE 2: OSCILLATION[1] IN RESPONDING

Group	N	Median[2]	Range
IA	4	4·5	4–7
II	8	10·0	3–35
III	8	12·5	6–18

[1] Trials during which animals jumped on and off the platform.
[2] The difference between Groups IA and III is significant ($p = 0.008$ two-tail test), but none of the other differences is significant.

It might also be expected that the introduction of conflict between the two avoidance responses would be associated with an oscillation of responding (Miller, 1944) evidence by jumping on and off the platform. Table VIII shows the median numbers of trials during which such oscillation occurred. Only the difference between Groups IA and III is significant, the rank order of the three groups being: IA, II, III. Table IX and Figure 4 show the breakdown of these overall data into

median oscillations per set of five trials. Again the performance of the general control group, Group IA, differs radically from the performances of the other two groups, very little change in response oscillation occurring during the 40 conflict trials.

TABLE IX

PHASE 2: CUMULATIVE MEDIAN NUMBERS OF TRIALS ON WHICH OSCILLATION
IN RESPONDING OCCURRED

Trials	Group		
	IA	II	III
1–5	1·0	4·0	4·0
6–10	2·0	7·0	6·5
11–15	2·5	8·0	8·5
16–20	2·5	9·0	9·5
21–25	2·5	9·5	11·0
26–30	2·5	9·5	11·0
31–35	3·0	10·0	12·0
36–40	3·0	11·0	12·0

FIGURE 4
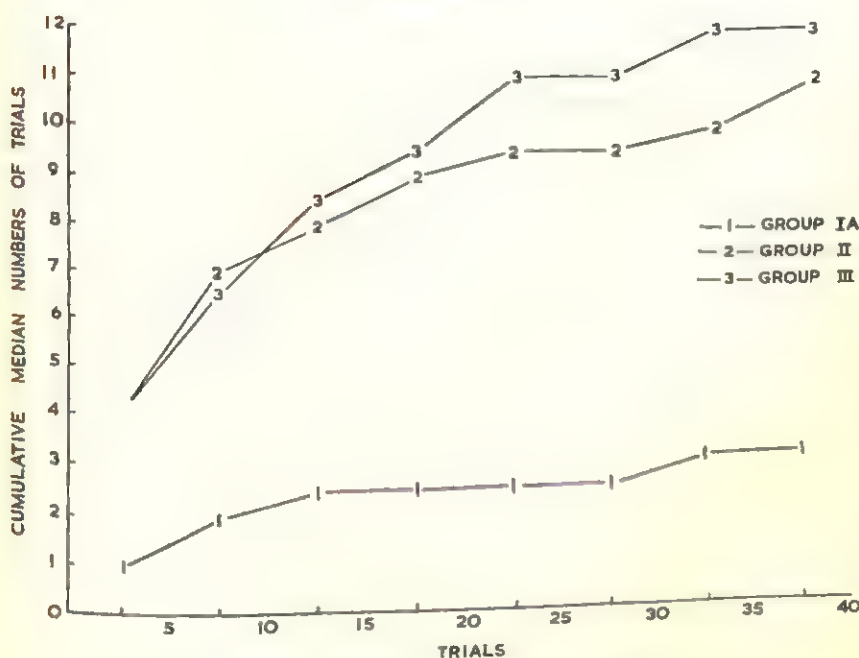
Phase 2: Oscillation in responding.

Groups II and III show relatively rapid increases in oscillation during the first half of this stress period, both tending to level off during the second half of the period and to diverge with Group II engaging in fewer oscillations. As with other reactions to conflict described above, those two groups, both having lower median food intakes and body weights, showed different trends in behaviour from that of the general control group.

A frequent concomitant of conflict between responses is the appearance of behaviour sometimes referred to as "displacement activity" (Russell, 1953). Such behaviour is "irrelevant" to the responses in conflict and may take a variety of forms.

TABLE X

PHASE 2: "DISPLACEMENT ACTIVITY"[1]

Group	N	Median[2]	Range
IA	4	0·0	——
II	8	9·0	2–24
III	8	0·0	0–4

[1] Trials during which the animal attacked the apparatus (biting).
[2] The differences between the two control groups (IA and III) and the thiamine-deficient group are significant ($p = 0.002$ and 0.001, respectively, one-tail tests); but the two control groups themselves do not differ.

FIGURE 5
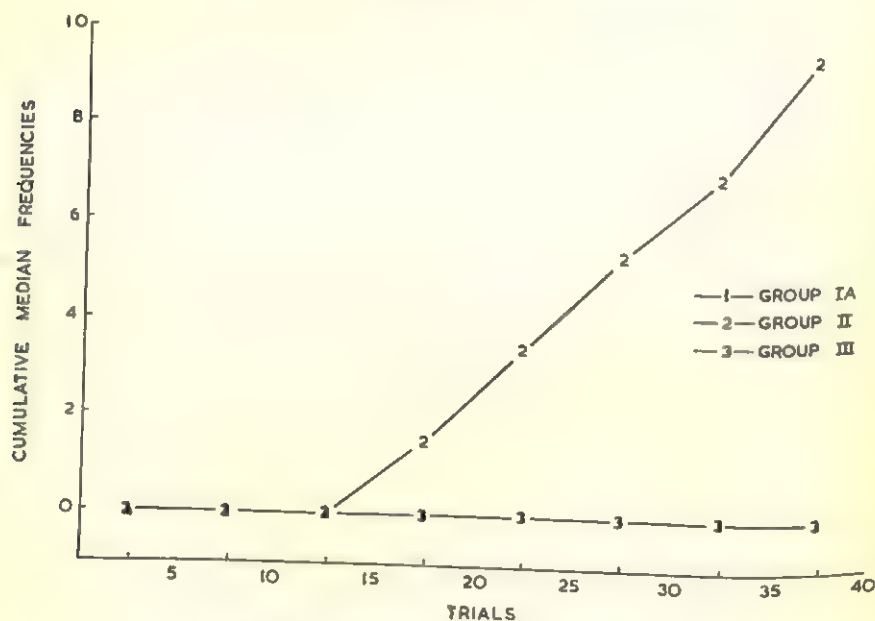
Phase 2: "Displacement activity."

The present conflict situation produced one very striking form of such behaviour in which the animal attacked the apparatus, biting it repeatedly during the conflict trials. Table X summarizes the frequencies with which this behaviour appeared for each of the various control and experimental groups. All the thiamine-deficient animals engaged in the behaviour during the 40 conflict trials, whereas none of the general control animals and only three of the caloric controls showed similar symptoms. The differences between the two control groups and the deficient group are both highly significant. Figure 5 shows the curves for the breakdown of the "displacement activity" scores into median frequencies per set of five trials.

Avoidance training 2

The after-effects of exposure to conflict may appear as decreases in the efficiency of learning a new response. In the present experiment this could be observed during the establishment of the second avoidance response involving lever-pressing.

TABLE XI

PHASE 3: NUMBERS OF ANIMALS REACHING AND FAILING TO REACH THE
CRITERION OF A CONDITIONING WITHIN FIFTY TRIALS

Group[1]	N	Conditioned	Not conditioned
IA	4	3	1
II	8	5	3
III	8	7	1

[1] None of the differences among these groups is significant.

TABLE XII

AVOIDANCE TRAINING 2: TRIALS TO REACH THE CRITERION FOR THOSE
ANIMALS CONDITIONING WITHIN FIFTY TRIALS

Group	N	Median[1]	Range
IA	3	22·0	17–40
II	5	11·0	1–33
III	7	7·0	1–23

[1] Differences among Group II and the two control groups, Groups IA and III are not significant ($p = 0.13$ and 0.22, respectively). The difference between Groups IA and III are significant ($p = 0.03$).

Table XI shows the numbers of animals in each of the various groups reaching and failing to reach the criterion of conditioning within the arbitrary limit of 50 trials. None of the differences is statistically significant. Comparisons, as summarized in Table XII, among those animals in the various groups reaching the criterion also give no significant differences between the thiamine-deficient group and the two control groups, I and III ($p = 0.13$ and 0.22, respectively). However, the caloric control animals as a group did condition in significantly fewer trials than the normal control animals ($p = 0.03$).

TABLE XIII

AVOIDANCE TRAINING: LATENCY OF RESPONSE

Group	N	Median[1]	Range
IA	4	64·00	37·0–70·0
II	8	49·25	9·0–70·0
III	8	11·00	6·0–29·0

[1] Differences between Groups IA and III and Groups II and III are significant ($p = 0.002$ and $p = 0.014$, respectively). Difference between Groups IA and II are not significant ($p = 0.29$).

Analysis of latencies in responding during avoidance training 2, summarized in Table XIII, shows the caloric control group to have been significantly faster in responding than either Groups I or II ($p = $ 0·002 and 0·019, respectively). The difference between the general control and the thiamine-deficient groups was not significant ($p = $ 0·29).

TABLE XIV

Avoidance Training 2: "Displacement Activities"

Group	N	Median[1]	Range
IA	4	0·0	–
II	8	3·0	0–27
III	8	0·0	0–I

[1] Differences between Groups IA and II and between Groups II and III are significant ($p = $ 0·024 and 0·01, respectively). The difference between Groups IA and III are not significant ($p = $ 0·077).

During training on this second avoidance response, six of the thiamine-deficient animals and one of the caloric controls showed the biting behaviour which had first appeared during the conflict trials. Table XIV indicates that this behaviour appeared much more frequently among the deficient animals, the differences among these animals and those of Groups I and III being statistically significant ($p = $ 0·02 and 0·01, respectively).

IV

Discussion

We will discuss these results in terms of the four main predictions which the research was designed to test.

Effects of thiamine deficiency on conditioning

The results of our first experiment indicated that thiamine deficiency of a degree not severe enough to produce polyneuritis had no significant effects on discrimination or on maze learning. On the basis of these observations we predicted that such a deficiency would not affect the efficiency of conditioning. The present experiment included two situations which provided data relevant to this prediction, both involving conditioned avoidance responses. The prediction was tested by comparing the thiamine-deficient animals with their corresponding caloric controls. In the first conditioning situation no significant differences were found for either the number of trials to condition or the latency of responding. In the second situation there was no significant difference in number of trials to condition, but the thiamine-deficient animals were slower in responding. These results generally support our prediction and are contrary to the observations of certain previous investigators, particularly Biel and Wickens (1941), who studied the effects of thiamine deficiency on the conditioning of eyelid responses. There are several possible explanations for these contradictory results, including differences in the method of producing the deficiency, in the level of the deficiency and in the form of conditioning involved. The possible influences of these and other variables were discussed in the report of our previous experiment (1956).

Effects of thiamine deficiency on reactions to conflict

We also predicted that the level of thiamine deficiency induced in the present experiment would not be associated with exaggerated reactions to conflict. This prediction was based upon the results of our previous study which showed no significant effects of the deficiency on reactions to the frustration of an insoluble problem situation. The present experiment provided four different measures of reaction to conflict: perseveration of previously conditioned responses, sometimes referred to as "response rigidity"; latency of responding; oscillation of responses; and "displacement activity," evidenced most strikingly in this instance as biting of the apparatus. The results showed significant differences between the thiamine-deficient animals and their corresponding caloric controls in all these measures except oscillation of responses. It is interesting to note that the deficient animals continued to show the "displacement activity," though much less frequently, in the second conditioning situation when they were no longer being exposed to conflict.

These conclusions arise from examinations of the overall performances of the two groups on the 40 conflict trials. When the data are broken down into performance during sets of five trials, more detailed pictures of behaviour trends emerge. For all four measures of reaction to conflict, the thiamine-deficient animals are seen to have responded initially in a manner very similar to their caloric controls. Divergences of the two groups become noticeable as the number of conflict trials increased. These divergences were in the direction of greater response rigidity and "displacement activity" on the part of the thiamine-deficient animals. Only in measures of response oscillation did any inconsistency with this general picture appear and in this case the difference in the overall performances of the two groups was not significant.

These results do not confirm the prediction we set out to test. They indicate that, under certain conditions but not others, impaired carbohydrate metabolism in the central nervous system resulting from thiamine deficiency may be associated with exaggerated reactions to stress. The exact characteristics of the critical conditions for producing this effect can only be determined by further experimentation.

Effects of reduced caloric intake on conditioning

Comparisons between measures of behaviour for the general and the caloric control groups made it possible to test predictions regarding the effects of reduced caloric intake on conditioning and on reactions to conflict. In our previous study we found no effects of such reduction on reactions to stress, but did obtain evidence that it was associated in certain circumstances with more rapid learning, even though the learning did not involve the food-hunger drive. We suggested that these differences in learning might be interpreted in terms of the effects of what previous investigators have referred to as "irrelevant drives." Our prediction in the present experiment was that conditioning would occur more rapidly in the caloric control than in the general control group because of interaction between the motivational effects of (a) the reduced caloric intake and (b) the noxious stimulus, avoidance of which served as reinforcement during conditioning.

Results of performance in the first avoidance-training situation showed that the general control animals as a group took more trials to condition and had longer response latencies than the caloric control animals. In fact, half of the former failed to reach the criterion for conditioning within the 100 trials provided. When those general control animals which did reach the criterion were compared with the caloric controls no significant differences were found either in the number of trials to condition or in the latency of responding, although in both measures the caloric controls tended to perform more efficiently. This latter trend was apparent in the analysis of

behaviour in the second avoidance-training situation and in this case the caloric control animals reaching the criterion for conditioning were significantly faster in responding and took significantly fewer trials to reach the criterion. These results support our prediction and confirm results obtained in our previous experiment.

Our prediction was made in terms of an hypothesised interaction of drives. However, there are other possibilities in accounting for the relative slowness of the general control animals to condition. It is possible that, being heavier than the caloric control and thiamine-deficient animals, they found moving in the restricted space of the apparatus more difficult. Another possibility is that they were less sensitive to the low intensity shock used. Although such possibilities as these cannot be ruled out without some additional research, it does appear to us that it would be difficult to account for all the results of our two experiments in terms of any of them. We believe these results suggest that "irrelevant drives" may have significant effects on performance and emphasize the importance of providing adequate controls in designing research in which such effects may be involved.

Effects of reduced caloric intake on reactions to conflict

The prediction that the two control groups would not differ in their reactions to conflict was supported generally by analysis of the overall performances of the two groups on the 40 conflict trials. No significant differences were found in response rigidity or latency, or in "displacement activity." However, the caloric control animals did show greater oscillation in responding than did the general controls.

In order to examine the trends in behaviour throughout the conflict period, the data were also analyzed in terms of performance during sets of five trials. The results of this analysis revealed differences in trends which were concealed in the treatment of overall data. In general they showed that, during the early conflict trials, the caloric control animals tended to perform in a manner almost identical with that of the thiamine-deficient animals. About midway through the conflict period there began to develop deviations between these two groups and from then on the caloric controls approached more and more closely the performance of the general controls. This suggests that reduced caloric intake affected reactions early during exposure to conflict and only became differentiated from the effects of thiamine-deficiency as the duration of exposure increased. It would appear important that future studies of reactions to stress should consider the possibility of effects appearing only after an initial lag and should analyze experimental observations accordingly.

The overall differences among the three groups, which indicate effects of reduced caloric intake and thiamine deficiency, are also reflected in the final order of the groups when ranked in terms of decreasing response rigidity, latency, and "displacement activity." In all instances the rank order for the conflict phase of the experimental design is: Group II (thiamine-deficient), Group III (caloric control), Group IA (general control). The differences in overall performances between Groups II and III are significant, but the differences between Groups III and IA are not. This suggests that these three aspects of behaviour are affected by thiamine deficiency. In contrast is the measure of response oscillation for which the final order is Group III, Group II, Group IA. In this instance the overall difference between Group IA and III is significant, but none of the other differences is. This suggests that this measure of behaviour is affected primarily by reduced caloric intake, and is consistent with the finding in our earlier study that animals on reduced caloric intakes were more active and showed more vicarious trial and error in situations in which the food-hunger drive was not being reinforced than animals fed *ad libitum*. When the present research design is employed, the ordering of the three groups may help in determining which

behaviour measures appear to be affected primarily by thiamine deficiency and which by reduced caloric intake.

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SOME ACUTE EFFECTS OF D-LYSERGIC ACID DIETHYLAMIDE ON THE RAT

BY

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There is a wealth of literature available on the psychosis-like effects of very small doses of *d*-lysergic acid diethylamide (LSD-25) in human subjects. It is reported that the drug produces profound changes in emotional behaviour, feelings of depersonalization, visual and auditory hallucinations, catharsis-like episodes and slight changes in autonomic nervous system activity.

The present experiment investigates the possibility of relating the behavioural disturbances induced by LSD-25 to known physiological data. Variation in the latency and running times of trained albino rats is examined. It is demonstrated that latency scores are directly affected by the cerebral concentration of the drug; and that sensory thresholds appear to be raised. It is further shown that when the level of stimulation is raised the rats appear to behave normally.

INTRODUCTION

On several species attempts have been made to assess the effect of LSD-25 on behaviour. Evarts (1956) using a drug-dosage level of 1·0 mg./Kg. body weight (0·001 mg./Kg. being sufficient to produce marked psychological disturbances in man) on eight immature monkeys demonstrated a decrement in the functioning of the senses. There appeared to be no specific defects in the musculature. Disturbance of locomotor and postural abnormalities were considered to be due to a sensory defect rather than a motor failure. More specifically the defect was considered to be in the proprioceptive feedback mechanism. Schwarz *et al.* (1956) using the techniques developed by Feldberg and Sherwood (1954) employed the cat as experimental subject. The injection of 15 μgm./Kg. body weight of LSD-25 into the lateral ventricle produced immediate reactions of restlessness and retching, again with no impairment of motor function. After some time the cats became drowsy and showed reduced responsiveness to petting and play. The electroencephalogram showed low-voltage fast activity with occasional slow waves, a pattern consistent with the relaxed state of the animal.

These two papers suggest that LSD-25 produces a syndrome that may be characterized as a gross sensory disorder with no marked effects on muscular capacity. No attempt however had been made to assess the relation between the intensity of the effect and the amount of drug administered, nor the course of action of the drug in time.

Winter and Flataker (1956) were probably the first to employ an objective procedure to measure the levels of performance following treatment with LSD-25. Essentially they were concerned with the time taken for an LSD-25-dosed rat to escape from a series of noxious stimuli. Their conclusions seem to indicate that the duration of the response bears a linear relationship to the log dose of the drug. Their study does not however make any distinction between the latency of the response and the actual duration of the response. None of the studies reported so far have offered information which could be said to link the pharmacological role of LSD-25 in the central nervous system and behaviour.

The present experiment was designed to discover first, the course of LSD-25 disturbance in the rat, i.e. the onset, duration, and clearance of the drug, using some behavioural criterion; and secondly, to investigate the nature of the locomotor and postural effects that the drug produces. Two measures were employed, latency and running time. Latency was taken to be the time required for the rat to organize its response to a highly trained situation. Such an activity may be expected to be interfered with by the drug in a manner consistent with the dose and time of injection. Running time was considered a suitable measure of locomotor efficiency. It was postulated that the locomotor disturbance could be produced by a non-specific sensory cut-out effect of the drug. An attempt was consequently made to offset the hypothetical raised sensory thresholds by increasing the sensory input in one modality, auditory being chosen for this purpose.

DESIGN

The experimental design is shown in Table I. The experiment was conducted in five replications. Each replication consisted of a litter of four animals, one animal being randomly assigned to each experimental treatment. Experimental Group IV comprised the control group, re-run with the lowest drug dosage.

TABLE I
ANIMAL GROUPS

Procedures	Control	Exp. I	Exp. II	Exp. III	Exp. IV
Day 1	Control	Control	Control	Control	Control
Day 2	Distilled water	Drug	Drug	Drug	Drug
Day 3	Recovering	Recovering	Recovering	Recovering	Recovering

↑   ↑
Control group re-run as Experimental Group 4

SUBJECTS

The subjects of the experiment were obtained from the Medical Research Council Toxicology Unit, Carshalton, Surrey. They comprised five litters, each of four male albino rats, obtained at weaning. The animals were uniformly handled and treated throughout the experiment. They were fed on the Approved Medical Research Council

TABLE II

Experimental groups	Drug dosage
I	500 μgm./Kg. body weight
II	250 μgm./Kg. body weight
III	125 μgm./Kg. body weight
IV	62.5 μgm./Kg. body weight

diet—diet 41B (Bruce & Parkes, 1949). At 80 days, the rats were placed on a two-hour feeding schedule. Preliminary training started at the age of 100 days, the mean weight of the rats being 250 gm. The rats were given the acute drug treatment on Day 2 of the

experimental procedure. The four experimental groups were given intraperitoneal injections, in four graded doses. The drug-dosage levels are shown in Table II. The drug volumes were made up with distilled water to 1·5 ml., the control groups received this volume of distilled water only.

PROCEDURE

The animals were trained to run a two-and-a-half metre runway to a food jar under hunger motivation. At subsequent periods during the training one-way non-retraceable doors were placed first at the starting end, to comprise a starting box, and second at the food end to constitute a feeding compartment. During training, the animals were run ten times each day, and permitted· to eat for 15 seconds at each trial. Training was regarded as completed when the total time, i.e. latency plus locomotion was not more than one minute, with no attempt at retracing the runway, on seven successive trials. At each trial the following times were taken—the time taken by the animal to organize its response and leave the starting box, and the locomotion time from the starting box to the food compartment. During the experiment the observations were made at 5, 10, 20, 30, 45, 60 and 90 minutes after injection, giving seven samples of the behavioural effects of LSD-25. On any trial if the animal had not left the starting box at the end of five minutes, a small noise generator was started. This was unfailingly successful in producing almost immediately a locomotion response towards the food compartment. When the rat passed through the non-retracing door of the food compartment, the stimulus was switched off.

Each replication was run over three successive days, the control day, the experimental day and recovery day. The experimental treatment may be seen in Table III.

TABLE III
EXPERIMENTAL TREATMENT

Groups		Control or Day 1	Drug or Day 2	Recovery or Day 3
Control	Preliminary	Latency	Latency	Latency
Experimental	Training	Running time	Running time	Running time

THE EFFECT OF DRUG TREATMENT ON BEHAVIOUR

The nature of the action of the drug is distinctive and gross. The particular syndrome evoked was common to all the animals tested, and there was no difference with different modes of introducing the drug. In this experiment intraperitoneal injection was preferred owing to the swiftness with which this can be carried out.

The drug produces a characteristic syndrome, with postural and sensory involvement. Many features of this syndrome are not unlike those reported in other species (Evarts 1956). The most prominent feature is postural. The rat tightly tucks all four limbs under the body, and makes agitated crawling movements, with the abdomen pressed to the floor. The rat will make numerous ataxia-like circling movements, with the body leaning against the confining walls. Rats have made as many as 9 such circular movements in 2–3 minutes. When handled the rat is unusually tame, though muscle tone is normal, and reflexes are unaffected. The only evidence for autonomic involvement was found in the highest drug groups, in which all animals defecated and urinated in the first ten minutes. The drugged rat will frequently finish its crawling movements with its head tightly pressed into a corner, and behaves as if it had some difficulty in finding its way out. The pupillary reflexes appear normal, as are the reactions of the pinnae to the slightest auditory stimuli. Reactions other than reflex to either visual or auditory stimuli are significantly absent. A drugged rat may be brought successively closer to a 100-watt light bulb without showing any resistance until an inch or so away. Even then the resistance takes the form of head deflection only. An inspection probe placed almost next to the eye

produces no reflex blink, nor any avoidance movement of the head. The drugged animals evidence no exploratory or orientation behaviour, and there is a complete absence of preening and body washing. Internal stimulation states also have little effect on LSD-25-dosed rats. Rats on 24 hour and even 48 hour hunger drive do not eat during the period of the drug effect.

RESULTS

The first part of the experiment was designed to demonstrate the differential effects of different levels of drug dosage of LSD-25 on latency and speed of locomotion in albino rats, in particular to demonstrate whether the onset, duration and clearance of the drug bears any relation to the dose.

The latency measure is the time taken by the animal in the starting box after being placed in the receptacle to leaving it via the non-retraceable door; f.e. the time taken by the animal to orientate itself and to organize its response to the food jar at the end of the runway. The data may be conveniently arranged in a progressive table showing a latency of response for 5 groups of 5 animals under different experimental and one control condition—the means being calculated on the time sample ranging progressively from 5 minutes to 1 hour 30 minutes after the injection. (See Table IV.)

TABLE IV
MEAN LATENCY IN SECONDS AT GIVEN TIME INTERVALS AFTER INJECTION

Time after injection of drug	Control distilled water N.5	Exp. I 500 μgm./Kg. LSD-25 N.5	Exp. II 250 μgm./Kg. LSD-25 N.5	Exp. III 125 μgm./Kg. LSD-25 N.5	Exp. IV 62.5 μgm./Kg LSD-25 N.5
5′	23·4	311·2	284·6	255·2	8·2
10′	13·4	316·8	316·6	261·0	83·0
20′	10·4	312·4	323·6	175·6	273·4
30′	10·4	296·6	168·4	127·8	80·6
45′	6·6	91·6	79·6	84·6	17·6
60′	8·8	26·0	12·8	37·4	6·6
90′	5·2	12·6	7·8	7·8	7·6

In examining Table IV it will be noticed that there are very few mean latency scores much over 300 seconds. This is due to the fact that at the end of 300 seconds a trial was regarded as concluded and a new experimental variable was introduced, that is noise, to test the hypothesis concerning the nature of the effects of the drug as distinct from its purely temporal activity. Figure 1 demonstrates the differential onset, duration and clearance curve comparing all experimental groups with the control over a period of 1½ hours. Although by inspection there would appear to be a highly significant difference in performance of the drugged groups, there are no convenient statistical methods for demonstrating a particular level of significance at a particular time. The difficulty arises out of the dependence existing between successive observations and the differential effect that could be evoked by the use of extra auditory stimuli. To circumvent these objections a test of significance between the total latency times of the five animals in experimental group III and the control group was undertaken. As the latency time of group III was experimentally unadulterated by the effect of the extra auditory stimulation, a rigorous comparison with the control group may be made. Using t test for unrelated means $t = 56.666$

with 8 degrees of freedom giving a high level of confidence beyond the 0·001 level.
By implication and inspection it can be readily seen that the systematic order of the
results of the four experimental groups is directly attributable to the action of the drug.
The results are conditional on the demonstration that there was no initial difference,
inherent in the latency scores of the various groups of animals on their control
performance. The latency scores of the control group, Day 1, and the latency
scores of the experimental group III control run, when tested for significance, give a
$t = 1·6$ with 8 degrees of freedom, making the assumption valid that the groups
started off as adequately equated. A one-way analysis of variance on the mean
control day performance of all animals in the four groups, revealed that there was no
specific difference between group or within group effect other than that introduced
by the variance of the common parent population F_3, $16 = 2/3$.

FIGURE 1

Mean Latency Times of all Groups.
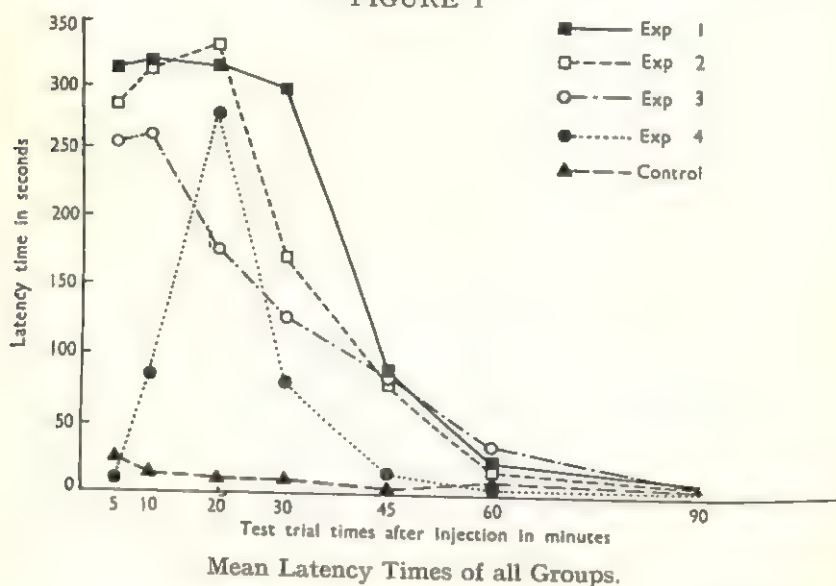
It will be seen by inspection of Figure 1 that at the end of the experimental period
all drug levels approach the control performance values. On day three there is no
significant difference between the control and experimental animals ($t = 0·51$ with 8

TABLE V

MEAN RUNNING TIME IN SECONDS AT GIVEN TIME INTERVALS AFTER INJECTIONS

Time after injection of drug	Control distilled water N.5	Exp. I 500 µgm./Kg. LSD-25 N.5	Exp. II 250 µgm./Kg. LSD-25 N.5	Exp. III 125 µgm./Kg. LSD-25 N.5	Exp. IV 62·5 µgm./Kg. LSD-25 N.5
5′	10·0	13·2	11·32	11·32	3·56
10′	8·2	6·96	9·96	9·98	31·64
20′	4·48	62·0	4·4	4·0	36·84
30′	6·4	7·4	5·32	5·2	56·2
45′	4·88	4·36	8·6	8·0	7·32
60′	4·2	14·44	8·36	8·0	4·24
90′	3·0	7·24	6·92	8·12	3·92

degrees of freedom).   An analysis of variance on the recovery day results produced an F_3, $16 = 1/3$, insignificant for both within and between groups.

The second part of the experiment indicates the effect of the drug on running times.   The data are set out in Table V.


FIGURE 2

Mean Running Time of all Groups.
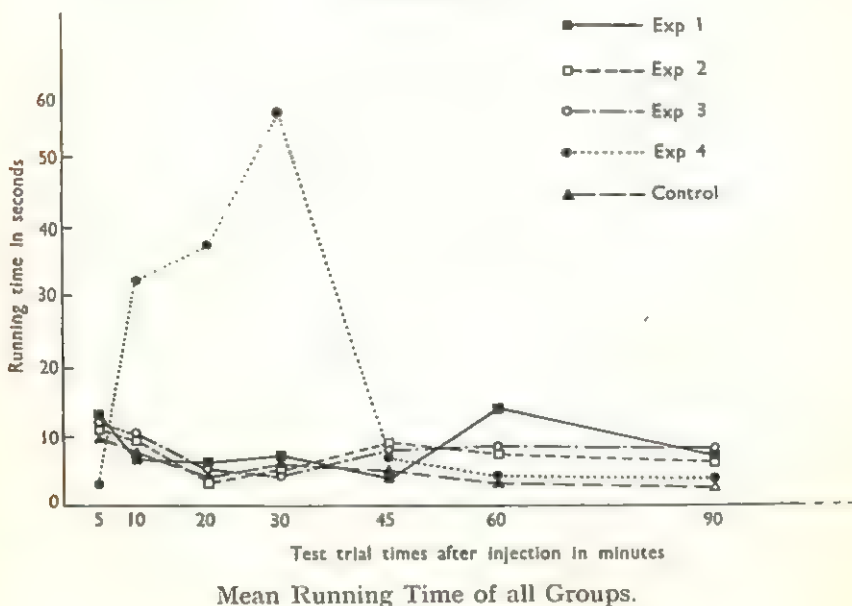
It is clear from inspection that the running times do not show any of the remarkable differences observed in the latency data.   Figure 2 shows the time taken to run two metres at given sample times.   Testing the significance of the difference between the mean performances of experimental group 1 and the control $t = 4.23$ with 8 degrees of freedom—making it possible to reject the null hypothesis at the 1 per cent. level.   By implication a greater significance exists for the decrement of the locomotor performance of the lowest drug group, in the initial 45 minutes of the experiment.

DISCUSSION

There was no significant statistical difference between the mean time in either latency or locomotion of any group on day one or day three.   That is, all experimental groups started off reasonably equated, and on the recovery day no effect of the drug could be detected.   Consequently, the discussion will be confined to the period during which the drug was effective, namely day two.   From the curves plotted for the four experimental groups, it would appear that the effects of LSD-25 on the latency scores have a reasonable relationship to the body-weight dose of the drug.   The onset of the drug's action is rapid.   After five minutes the latencies of the four experimental groups are distributed in accordance with the drug dosage levels.   The lowest drug group rats, however, show a remarkably short latency, even below that of the control group.   This can probably be accounted for by the intense bursts of activity which LSD-25 treated rats display in the first few minutes immediately after injections.   In the case of this group, the relative smallness of the dose may account for the longer period required to produce a build up of the drug

in the brain.   Hence the lack of effect on latency time until such a build up has occurred.   The suggestion that in this particular case, the latency scores do indicate the concentration of cerebral LSD-25 is borne out by observations made with 14C-labelled LSD-25.   Stoll, Rothlin, Rutschmann, and Schalch (1955), injected suitably prepared 14C-labelled LSD-25 (50 μgm.) into the tail vein of mice.   These were then decapitated at from 10, 30, 60 and 120 minutes after injection.   Their observations showed that practically the whole radio-activity of 14C labelled LSD-25 disappears from the blood within a few minutes, whereas most organs, including the brain, reach their highest levels only after 10–15 minutes (see Fig. 3).   Particularly note-worthy is the similarity of shapes between 14C-labelled LSD-25 count per minute curve, and that of the effect of the drug on latency over the same time basis.   The similarity of the curves, however, is not unique to 14C LSD-25.   Lanz, Cerletti and Rothlin (1955), using a bio-assay technique, have plotted tissue concentration figures for LSD-25.   The brain tissue concentration of LSD-25, against a common time base line, in general agree with the finding in the previously mentioned study.
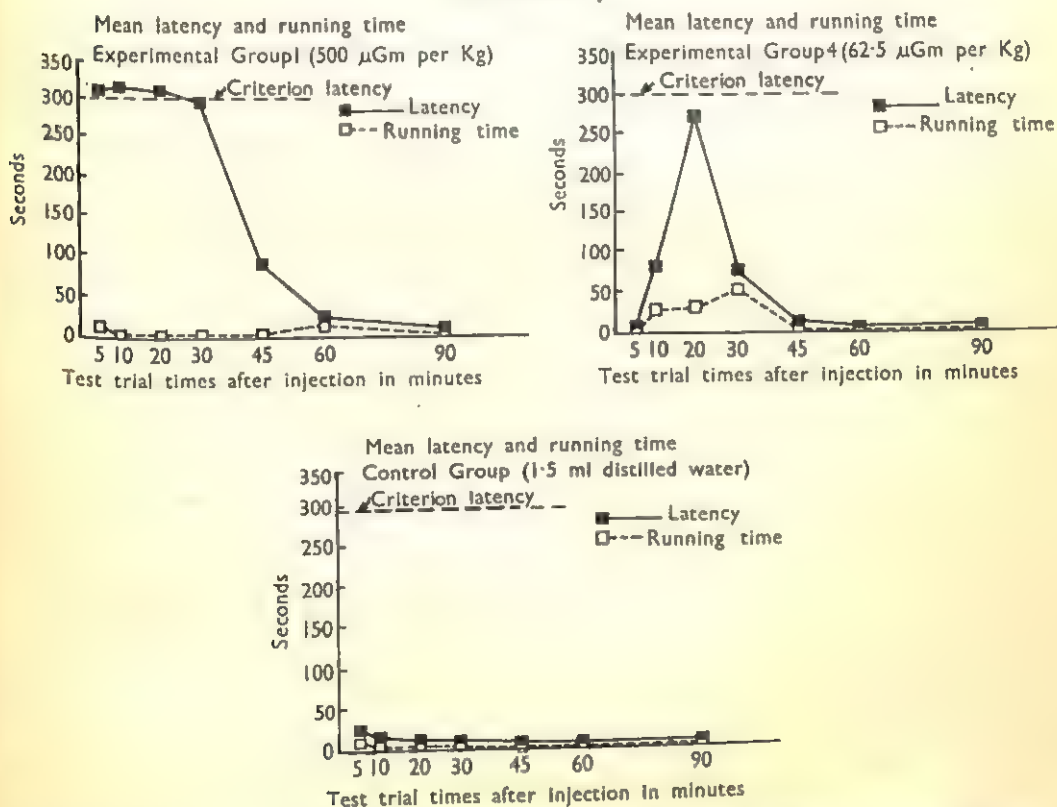
FIGURE 3

Test trial times after injection in minutes
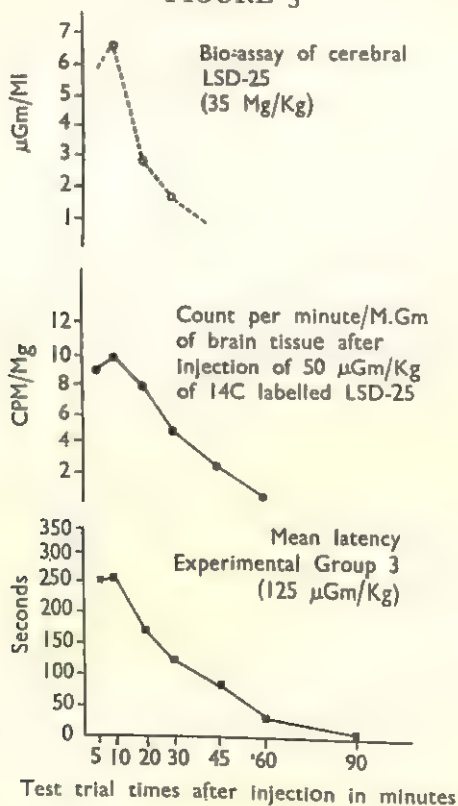
The second observation that could be discussed is concerned with the locomotor aspect of the drug's action.   Woolley (1955) has reported that LSD-25 produces a marked syndrome in mice, characterized by walking backwards.   Evarts (1956) has reported an extraordinary postural component in the behaviour of monkeys.   It has already been pointed out in the present report that LSD-25 produced a marked postural and locomotor syndrome in the rat.   After a latency of five minutes, a noise

generator was activated in the starting box. This procedure was sufficient to produce the appropriate running response to terminate a trial—appropriate in the sense that the animal moved out of the starting box, down the runway to the food box, maintaining what would appear to be normal contact with the situation, until the extra auditory stimulation cuts out. The significant observation was the speed with which this was accomplished. The speed of locomotion with extra stimulation, was well within control expectation, whereas without the stimulation, it was significantly different. Figure 4, shows that by 45 minutes the latency scores are almost normal—in both for the highest drug group and the lowest—two extremes of the drug dosage continuum. Only the highest dosage group reached the criterion of five minutes in the first 45 minutes of the experiment, and hence, experienced the extra auditory stimulation. In contrast the lowest drug group, which did not receive the extra auditory stimulation, shows running times significantly longer than the controls in the first 45 minute period, and normal for the second.

FIGURE 4

Mean latency and running time
Experimental Group1 (500 μGm per Kg)

Mean latency and running time
Experimental Group4 (62·5 μGm per Kg)

Mean latency and running time
Control Group (1·5 ml distilled water)

Some general conclusions can perhaps be put forward. Starting with the hypothesis that LSD-25 interferes with the normal reactive capacity of the rat, and selecting a suitable behavioural index, it has been possible to demonstrate that performance capacity varies with the concentration of the drug. Some support for the contention that latency may be a suitable index to the concentration of cerebral LSD-25 can be obtained from independent physiological, and bio-assay techniques. The second hypothesis tested in this experiment concerned itself with the problem as to whether the observed postural abnormalities could be ascribed directly to the action of the drug. The locomotor speed times tend to argue that the drug has no

direct effect on the efferent system. When external stimulation is increased the animal has, what appears a normal reactive capacity. Thus it is possible that the drug impairs the cerebral mechanisms which translate the afferent and somatic impulses to the efferent areas. Alternatively, the action of the drug may be to interfere with the activity of the afferent system as such. The evidence here is confusing and often contradictory. Purpura (1956) has found that the effect of LSD-25 administered on the specific afferent system in the cat, depends on drug concentration. Extremely small dosages (2 μgm./Kg.) produces facilitation of evoked auditory and visual primary response in unanaesthetized, paralyzed cats. A higher concentration (40 μgm./Kg.) produces differential depressions of evoked auditory responses, with continued facilitation of the evoked visual primary resonses. In contrast is the observation of Rovetta (1956) who demonstrated negligible effects of LSD-25 on evoked response either auditory or visual. However, the fact that the latter experimenter applied the drug topically instead of intravenously, might account for the difference in their results. Evarts *et al.* (1955) injected LSD-25 via the intra-carotid route in the cat. He determined that the drug blocks or reduces the post synaptic spike in the lateral geniculate nucleus, but does not decrease the cortical response to optic radiation stimulation. Large dosages (1·5 mg. LSD-25) decrease the optic tract response to photic stimulation of the retina and abolished spontaneous cortical activity. From Evarts' results, it is likely that "the thalamic relay station" is quite sensitive to LSD-25 according to the level of drug concentration.

In view of the fact that increased levels of sensory stimulation produce responsive behaviour it is possible to assume the likelihood that LSD-25 acts as an enzyme or bio-electric inhibitor at certain synaptic junctions. The interference with specific enzyme systems may differentially affect normal neuronal conduction, a condition which may to some extent be offset by increased stimulation.

It is suggested that experiments of the kind reported here may be used to investigate the biochemical models that have been developed to explain the action of LSD-25.

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TRANSFER EFFECTS IN REACTIONS TO "STRESS"

BY

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In a previous investigation we showed that, when human subjects were exposed to the stress of an insoluble problem while under the influence of nitrous oxide, the usual adverse effects of the stress on subsequent learning were abolished. We suggested that these empirical results might be accounted for either in terms of "anxiety-reduction" or in terms of transfer effects.

The results of the present experiment show that differential transfer effects do occur in the insoluble problem situation when the degree of similarity of this task to the subsequent soluble task is varied. They also show that inhalation of nitrous oxide during exposure to the insoluble task abolishes these differential transfer effects, as well as any other effects which may be interfering with subsequent learning. These other effects, if they are present, may include "anxiety" and other "unique properties" referred to by other investigators using the insoluble problem situation in experimental studies of stress.

INTRODUCTION

In previous experiments (Russell & Steinberg, 1955) we studied the effects of a small dose of the anaesthetic drug nitrous oxide on experimentally induced "stress" (Russell, 1953) in human subjects. An "insoluble" temporal maze was used to induce stress, and its effects were measured on the subsequent learning of a similar but soluble maze. We found that nitrous oxide and exposure to stress, taken separately, each impaired learning of the soluble maze. But when subjects were exposed to stress while under the influence of the drug, the effects of stress on subsequent learning were abolished.

Two principal interpretations of these results were possible, the first in terms of "anxiety reduction." Earlier research (Steinberg, 1954) with the same concentration of nitrous oxide has shown that the drug produces mild euphoria and reduces overt signs of anxiety. It may have been, therefore, that reduction of these normal reactions to stress eliminated their subsequent interference with learning. A second interpretation was however possible in terms of conventional learning principles, i.e. negative transfer effects. The insoluble and soluble mazes used were similar in form and were presented in a similar manner. The effects of practice in the insoluble maze may have transferred to subsequent learning of the soluble maze. Both interpretations are based upon the principle that exposure to the insoluble task interferes with learning of the subsequent soluble task. They differ in regard to the nature of the interference effects, i.e. whether these are the "unique properties" (Maier, 1949) of exposure to stress or the more direct effects of transfer of practice.

One way of discriminating between these alternative interpretations would be to compare the influence of nitrous oxide on the learning of a soluble problem following exposure to stress when the magnitude of the transfer effects is varied. This magnitude is a function of both stimulus and response similarity between task 1, the insoluble maze in our case, and task 2, the soluble task (Osgood, 1953). In our first experiment the stimulus and response characteristics of the two tasks were very similar; conditions under which transfer effects would be expected to be maximized. If a soluble task were used in which the stimulus and response characteristics were very different from those in the preceding insoluble task, transfer effects would

tend to be minimized. By presenting the same insoluble task as before and so exposing subjects to the same stress, while varying the degree of transfer in this way, it should be possible to determine whether transfer effects are involved in this type of experimental situation. If transfer effects are involved, it would then be possible to study how they are affected when exposure to the insoluble task occurs under nitrous oxide. This is the basis upon which the present experiment was designed. The insoluble problem situation is used frequently in experimental studies of stress. These questions have a direct bearing on the interpretation of their results.

EXPERIMENTAL DESIGN AND PROCEDURE

Experimental design

The design of the experiment is shown in Table I. It was identical with that of our previous experiment, except for the nature of the soluble task in phase 3 which now consisted of learning nonsense syllables instead of a temporal maze. Two groups of subjects were used. Both were first required to respond at 50 choice points of the insoluble maze and then learned a list of 15 nonsense syllables by the serial anticipation method. The experimental group inhaled the drug while working on the insoluble maze and air while learning the syllables; the control group breathed air throughout. All subjects breathed through a face mask during the entire experiment.

TABLE I
EXPERIMENTAL DESIGN

Group	Phases		
	I	2	3
	Addition	Insoluble maze	Nonsense syllables
Experimental ..	Drug	Drug	No drug
Control	No drug	No drug	No drug

Subjects

Sixteen subjects were assigned at random to two groups of 8 subjects each. All were male medical science student volunteers aged between 18 and 25.

Apparatus

The apparatus for administering the drug and for presenting the insoluble maze was the same as that used in our previous experiments (Russell & Steinberg, 1955). The list of nonsense syllables and the method of presenting them have been described by Steinberg and Summerfield (1957).

Administration of drug. All subjects breathed through a face mask connected by rubber tubing to gas flow-meters. Those receiving the drug breathed 30-per cent. nitrous oxide in oxygen, scented with lavender; controls received similarly scented air.

Insoluble maze. The apparatus for presenting the temporal maze consisted of two spring-loaded keys operated by the subject, and two signals: a white light and a buzzer. The signals were controlled by mechanically operated switches which could be set in predetermined sequences by the experimenters. For the present purpose they were set in a sequence of 10 pressings which could be repeated, viz. R-L-L-L-R-R-L-R-R-L. This sequence was sufficiently long and complex for subjects not to solve it within the five repetitions presented. Correct responses were signalled by the flashing of the white light and incorrect responses by the buzzer.

Nonsense syllables. Fifteen 3-letter nonsense syllables, association value 50 per cent (Glaze, 1928), were recorded letter by letter on magnetic tape. The tape was played repeatedly by means of an "endless loop" attachment and heard by both subject and experimenters through earphones. The serial anticipation method was used.

Procedure

The general procedure was identical with that used in our previous experiment, except for phase 3 where nonsense syllables were substituted for the temporal maze, and was the same for both groups of subjects.   Before phase 1 began, subjects were given instructions for the tasks they were to carry out during the experiment.   Standard questions were asked to ensure that the instructions were understood.

During the first 5 minutes of inhalation through the mask all subjects worked on simple addition problems.   This period allowed the drug to take effect in the experimental group, the effects being shown by decreases in the number of correct additions and increases in errors.

The subjects then began work on the temporal maze.   The length of the sequence was never disclosed.   It was repeated five times, requiring each subject to make decisions at 50 choice points.

The final phase of the experiment followed.   The experimental subjects were shifted from inhalation of the drug to air, without being informed of the shift.   Pharmacological evidence has shown that the after-effects of the drug at this concentration disappear rapidly.   Fifteen trials of the list were given, followed by an interval of $2\frac{1}{2}$ minutes during which subjects were asked questions unconnected with the learning task, and finally a further five trials with the list.   The $2\frac{1}{2}$ minute interval was introduced to make the procedure comparable with that used by Steinberg and Summerfield (1957).
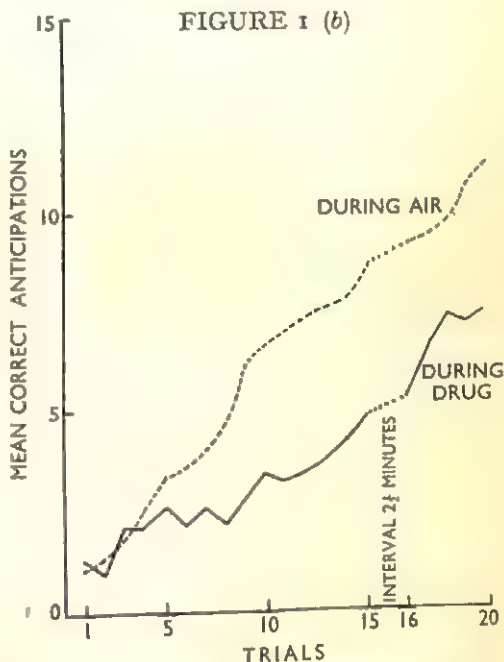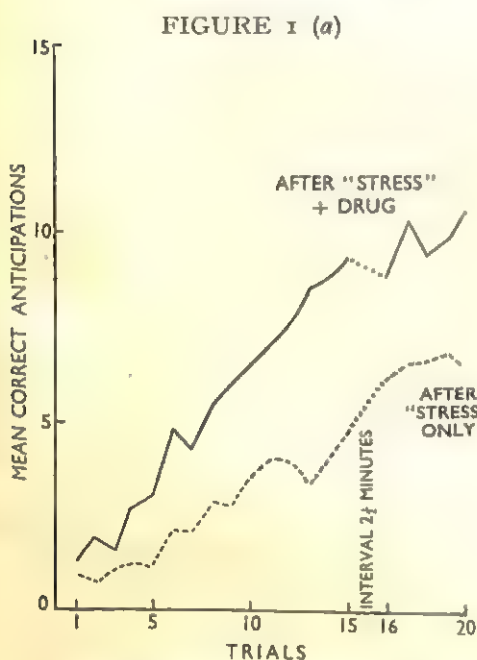
Mean correct anticipations on each of 20 trials: (a) Effect of "stress" on learning (present experiment), and (b) Effect of drug on learning (Steinberg and Summerfield, 1957).

Results

Effects on learning nonsense syllables

In Figure 1 (a) the mean number of correct anticipations on each of the 20 trials has been plotted for the two groups.   It will be seen that the experimental group which had breathed nitrous oxide during practice on the insoluble maze learned more quickly than the control group.   Table II shows the experimental group made significantly more total correct responses during the 20 trials, and by the twentieth trial had learned nearly twice as many syllables as the control group.   Inspection of

the learning curves suggests that the control group lagged behind the experimental group by an equivalent of about six trials.

<div align="center">TABLE II</div>
<div align="center">PERFORMANCE IN LEARNING THE SOLUBLE TASK</div>

Group	n	Total correct anticipations, Trials 1–20		Correct anticipations, Trial 20	
		Mean	Range	Mean	Range
Experimental	8	131·88	64–232	11·75	6–15
Control	8	72·50	15–153	6·50	2–12
P, one tail <		0·041		0·019	

Analysis of transfer effects

The results of this experiment have the same overall trends as those in our previous investigation. The effect of inhaling nitrous oxide during the insoluble task as compared with inhaling air was significantly quicker learning of the subsequent soluble task. This effect is particularly striking when the learning of the soluble task by the present experimental group is compared with learning of the same task by subjects having no pre-exposure to drug or stress. Such a comparison can be made using data reported by Steinberg and Summerfield. It shows that the two groups do not differ significantly (P > 0·05); the drug seems to have abolished the usual after-effects of exposure to stress, as they appeared, for example, in the present control group. The comparison is shown graphically in Figure 1.

Although the overall results are similar, our two experiments diverged in only one important respect; the soluble tasks were different. In both experiments the stimulus and response characteristics of the soluble task varied from those of the insoluble, but in the present experiment the two tasks were less similar than were the two tasks in the previous experiment. We would expect transfer effects to diminish as this similarity decreased. Comparing the effects of nitrous oxide and air on performance at these two levels of transfer should provide information on whether transfer effects are involved in the action of the drug under our experimental conditions.

To carry out such an analysis it is necessary to transform measures of performance on the soluble tasks into comparable units. Therefore the learning scores were expressed as the ratio:

$$\frac{X}{\bar{X}_C}$$

where X is an individual subject's learning score, and $\bar{X}_C$ is the mean score for learning of the soluble task without pre-exposure to drug or stress.

Table III gives these derived measures. Several tests for the significances of the differences between the groups have been tried. All give similar results and we report here the probability levels given by the Kolmogorov-Smirnov test (Siegel, 1956) since this test is sensitive to changes in the shape of distributions as well as to changes in central tendencies. The difference between the groups performing the insoluble task under the air conditions is significant (0·05 > P, one tail, > 0·01), i.e. transfer effects were greater where the insoluble and soluble tasks were more

similar. The corresponding difference under the drug conditions is not significant (P > 0·05).

TABLE III
ANALYSIS OF TRANSFER EFFECTS: DERIVED MEASURES*

1. *Air during performance of insoluble task.*

Group 3 Experiment 1	Group C, Experiment 2
0·671	1·097
4·624	1·565
1·364	1·599
3·052	1·314
3·283	1·075
2·844	1·628
1·965	0·840
0·231	1·428

$$0·05 > P_{\text{1-tail}} > 0·01$$

2. *Drug during performance of the insoluble task.*

Group 5, Experiment 1	Group E, Experiment 2
0·694	0·457
1·364	1·348
0·740	1·182
1·041	0·919
0·439	1·085
0·647	1·062
1·156	0·388
0·347	1·239

$$P_{\text{1-tail}} > 0·05$$

* $\dfrac{X}{\overline{X}_C}$ where X is an individual subject's learning score in the soluble task, and $\overline{X}_C$ is the mean score for learning the soluble task without pre-exposure to drug or stress.

DISCUSSION

The following argument can now be advanced.

(i) The only difference between the two experiments was the degree of similarity of the two soluble tasks to the insoluble task. This difference was reflected in significant differences in performance of the two soluble tasks following exposure to the insoluble task while breathing air, the task with the greater similarity—the soluble maze—showing the greater transfer effects. Thus it appears that the insoluble task situation, in terms of which stress was defined, induces transfer effects as well as symptoms of "anxiety" and other "unique properties" which ". . . make frustration induced behaviour different in kind from that produced in a motivated state" (Maier, 1949).

(ii) Performance of the soluble tasks following exposure to the insoluble task under drug conditions was the same as the performance of groups who learned the tasks without pre-exposure to drug or the insoluble task.

(iii) There was no significant difference between performances on the soluble task in the two experiments when exposure to the insoluble task was combined with inhalation of the drug. The drug abolished the differential transfer effects of pre-exposure to the insoluble task and resulted in a level of performance equivalent to that of subjects having no experimental treatments whatsoever prior to learning the soluble task.

In summary, we have shown, first, that differential transfer effects occur in the insoluble task situation when the degree of similarity of this task to the subsequent soluble task is varied; and, secondly, that inhalation of nitrous oxide during exposure to the insoluble task abolished these differential transfer effects, as well as any other effects which may have been interfering with subsequent learning. These other effects, if they are present, may include "anxiety" and other "unique properties" referred to by other investigators using the insoluble problem situation in experimental studies of stress.

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MISCELLANEA

A SIMPLE METHOD OF FIXING CHRONICALLY IMPLANTED ELECTRODES IN THE BRAINS OF SMALL ANIMALS

BY

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It is difficult to fix electrodes implanted into the brain of a small animal like the rat. If localization is to be accurate no movement of the electrode must occur between its implantation in the brain and fixation to the skull. The method of Olds (Olds, J. and Milner, P., 1954) employs a small plastic plate to which the electrodes are attached before insertion, the plate being screwed to the skull once the electrode has been implanted. Whilst ensuring firm fixation, this technique is difficult to perform without moving the implanted electrode during screwing of the plate to the skull.

The following method overcomes this difficulty, and allows very accurate placement of the electrode and fixation without the least disturbance. A small hole is drilled in the skull (from which the periosteum has been cleared) at the intended point of entry of the electrode. Around this and separated from it by 2·3–3·0 mm. are drilled three further holes, into which oversize watch screws are screwed with their heads remaining somewhat above the skull surface. The electrode is then lowered stereotactically into the brain, the dura having first been punctured. Whilst the electrode is still in the stereotaxic instrument, a rapid setting dental acrylic (e.g. Duzall, Coralite Dental Products, Chicago, U.S.A.) is flooded around it and the three screws. When the cement has hardened the electrode may be removed from the stereotaxic instrument, and will be found firmly anchored in place. It is then bent at its point of exit from the plate of acrylic, another drop of cement placed on the bend and allowed to harden. Then it is bent vertically again, the insulation stripped off and a short piece of hypodermic tubing is placed over it. The tubing is anchored in place with more cement, and the skin wound lightly closed over the assembly. Finally, the tubing is pinched with small pliers to ensure that it makes contact with its contained electrode.

This method provides firm and durable fixation, and we have not observed any irritative effects of the acrylic either on the skin or the brain. (Provided the hole in the skull through which the electrode is inserted is small, the acrylic does not enter the cranium at all.) Animals will tolerate the assembly up to six months, and maybe for a longer period.

We have only encountered two difficulties. Animals kept in wire mesh cages tend to wrench off the assembly if they get it caught in the mesh, and to avoid this we keep our preparations in smooth-sided cages. Also, the polymerising fluid used with the acrylic dissolves most conventional varnishes used to insulate wire. To overcome this we have used electrodes with glass insulation. A short length (12 in.) of nichrome wire (0·1 mm. diameter) is placed in a glass tube of 3–4 mm. bore. The tube is heated and rapidly drawn out to form a fine capillary enclosing the wire. In this way straight electrodes of a diameter of as little as 0·15 mm. may be made. Bipolar electrodes can be made by lightly cementing two of these together.

We have been using this method of implanting bipolar electrodes to determine the effect of stimulation of subcortical systems in freely behaving animals. The animals tolerate the electrodes and attached leads without any disturbance of their behaviour— so little are they affected that female rats will rear healthy litters after operation. The glass electrodes produce little histological change in the tissues, and their tracks are very small.

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CHANNEL CAPACITY AND REPETITION OF SIGNAL

BY

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Several recent experiments arising out of attempts to apply information theory to psychological phenomena have involved subjects performing tasks as rapidly as possible without making errors. The full implication of this condition has not so far been considered, largely because the mechanism has been conceived as a simple communication channel, whereas in fact an essential coding has taken place, implying the existence of a transducer.

It will be recalled that, whatever the entropy rate of the input, transmission in the presence of noise cannot exceed channel capacity C: whenever the input is in excess of this rate, errors occur at just such a rate as to hold the transmission rate down to C. On the other hand, by ideal coding, the maximum rate C may be realized in error-free reception. The efficiency of a coding system may be judged by the degree of error-reduction for a given amount of redundancy. Repetition of a message is a means of introducing redundancy, thereby reducing transmission errors, but is in general, inefficient compared with coding relying on the high probability of certain classes of long sequences of symbols. The latter coding does not appear to be relevant to situations in experimental psychology, where a response is demanded to each individual stimulus, and the question of sequences of sufficient length to make such coding efficient does not arise.

In that case, simple repetition is an efficient method of noise reduction, and is the mechanism suggested here. Suppose that one out of n equally likely stimuli are presented, and an agreed uniquely corresponding response has to be made. The response is made by the subject after the minimum delay to ensure a *correct* response. It is difficult to avoid introducing the concept of subjective probability at this point. Suppose the response time is split up into discrete quanta in each of which the signal is repeated, and a *potential* response is "stored." Stored responses are reliable, in the information sense (Goldman, 1953), to an extent $p < 1$, but the sequence of "stored" responses to repeated signals is reviewed continuously until the recurrence of one particular potential response exceeds the probability threshold, and the potential response becomes actual. Following Goldman, if the stimuli are $x_1, ----, x_n$ and the corresponding responses are $y_1, ----, y_n$, suppose the stimulus x_1 is repeated five times, and y_1 is the potential response three times, then the reliability of y_1 may now exceed threshold. Thus:

Stimuli	Potential responses	Actual response
x_1, x_1, x_1, x_1, x_1.	y_1, y_i, y_1, y_i, y_1.	y_1.

The mechanism of review of the potential responses is not postulated at the moment, although several likely ones may be suggested. The *essential result of the introduction of redundancy is a reduction of the information per symbol to the channel capacity*. Thus suppose the stimulus is transmitted r times, the entropy of the r-fold repetition of the symbol has the entropy of the symbol itself, $\log_2 n$, so that the entropy per symbol has dropped to $\frac{1}{r} \log_2 n$. It is suggested that r is determined subjectively to render $\frac{1}{r} \log_2 n$ equal to C. We thus have

$$r = \frac{1}{C} \log_2 n,$$

and r is also the time taken for the response. This may explain the result obtained experimentally by W. E. Hick (1952), and allow an interpretation of the constants of the equation obtained there.

Moreover, the action potentials may be interpreted as the mechanism of signal repetition. On this assumption, it would be natural for the increase in firing rate following increase in light intensity to reduce reaction time since now the information per symbol will drop to C in a shorter time. In fact, if impulse frequency is proportional to the logarithm of the stimulus intensity, the logarithmic relation between stimulus intensity and reaction time (Woodworth (1939)) would be a consequence.

A transducer may also eliminate the effect of noise by reduction of alphabet size (Goldman (1953)). This is suggested as an explanation of the anomalous results obtained by Fitts *et al.* (1956). It is hard to justify Fitts's contention that the choice of one out of a set of six figures of one kind provides more information than the choice of one out of a different set of six figures: as far as the subject is concerned the two sets of six constitute the respective populations. However, the notion of information in a display does seem a valid one, and the time for recognition of figures with high information content should be high, in agreement with Hick's findings. Now consider the possibility of alphabet reduction. This will decrease the information per symbol, and allow speedier processing of the message. For Fitts's constrained figures, a simpler alphabet may be proposed because there is greater variability from one to another than there is for his random figures. Thus consider the set of random numbers by which his constrained figures are produced. The set has a constant mean of 4·5 and constant sample variance of 6 square units. On the other hand, the set of numbers giving rise to a random figure, although having over all samples the same average mean and estimated variance, has for each particular sample a mean and a variance which may be widely different from expectation. This variability gives rise to distinctive features of random figures which are readily identified and so may issue in a reduced alphabet.

In more elementary figures, such as those consisting of one-dimensional stimuli discussed by Crossman (1955), the possibility of alphabet reduction does not exist.

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